S 213-A24

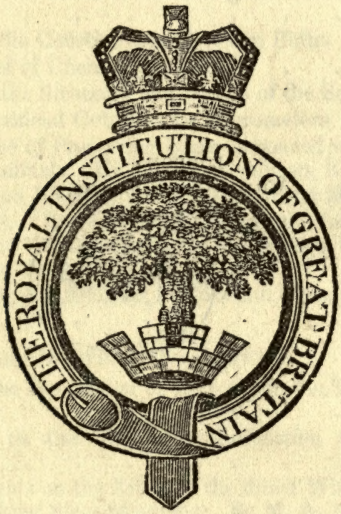
THE

QUARTERLY JOURNAL

BRITISH MUSEUM
OF
NATURAL HISTORY BRANCH

SCIENCE,

LITERATURE, AND ART.



JANUARY TO JUNE, 1827.

LONDON:

HENRY COLBURN, NEW BURLINGTON-STREET.

MDCCCXXVII.

LONDON:
WILLIAM CLOWES, PRINTER,
Stamford-street.

BRITISH MUSEUM
NATURAL

CONTENTS OF THE VOLUME.

No. I.

Sketch of the Life of the late Samuel Parkes, Esq. 1
On the Distribution of the Royal Society's Honours . . . 11
Essays and Gleanings on Naval Architecture and Nautical Economy, *reviewed* 18
On the Division of Labour as applied to Literature and Science . 37
Mr. J. Lindley on the Orchideous Plants of Chile 43
A Dictionary of Chemistry and Mineralogy. By William Campbell Ottley, *reviewed* 54
M. Gimbernat on the Mode of Extracting the Nutritious Portion of Bones 57
Hints respecting the Construction of Warm Baths . . . 62
Dr. Fyfe's Manual of Chemistry, *reviewed* 70
Dr. Harwood on the Structure and Habits of the Seal . . . 71
Dr. Goring on Artificial Cobwebs for Micrometers . . . 81
On the Importance of Preserving Facts connected with the Natural History of Animals. By William Swainson, Esq. 83
Outlines of Human Phisiology. By Mr. Herbert Mayo, *reviewed* . 91
Professor Renwick's Report on the Subject of Weights and Measures, made to the Commissioners for revising the Laws of the State of New York 101
On the Manufacture of Dies for Medals and Coinage. By Professor Brande 116

Astronomical and Nautical Collections.

i. Table of the Depression of Mercury in the Tube of a Barometer 122
ii. Remarks on the Change of Inclination of the Dipping Needle 207
iii. Experiments on the Effect of the direct White Light of the Sun in making Steel Magnetic. By M. A. Baumgärtner, of Vienna 125

The Life of Dr. Jenner, *reviewed* 141
Illustrations of the Elementary Principles of the Structure of Language 163
An Account of the Arcs perpendicular to the Meridian, which are now measuring on the Continent of Europe . . . 171
On the probable Decomposition of certain Gaseous Compounds of Carbon and Hydrogen, during sudden Expansion . . 204
Proceedings of the Royal Society 207
Proceedings of the Royal Institution of Great Britain . . . 209
Proceedings of the Horticultural Society 217

MISCELLANEOUS ARTICLES.

I. Mechanical Science.

Page

1 Micrometrical Measurement of Saturn and of Jupiter and its Satellites 220
2 Diamond Microscope 221
3 Results of Observations on Falling Stars 222
4 Experiments on the Resistance of various Bodies to Rupture caused by Longitudinal Tension 223
5 Magnetism of Watch-works corrected 223
6 Peculiar Construction of Shot-cartridges 224
7 Elastic Mould for Castings.... 225
8 Leslie's Apparatus for ascertaining the Specific Gravity of Powders 226
9 Menstruum for Biting-in on Plates of soft Steel ..... *ib.*
10 Etching on Steel..... *ib.*
11 Method of Cutting Glass 227
12 Preservation of Skins..... *ib.*
13 Indian Method of Varnishing.. *ib.*

II. Chemical Science.

1 Heat evolved from Air by Compression..... 228
2 On the Action of a Moving Metallic Disc, upon a portion of a Voltaic Conductor ..... *ib.*
3 Influence of the Aurora Borealis on the Magnetic Needle 229
4 Electro-chemical Appearances *ib.*
5 Earthy Depositions from Water influenced by the Electricity of Metals in contact 230
6 Instance of a returning Stroke by Lightning ..... *ib.*
7 Phosphorescence of Fluate of Lime 232
8 Chemical Compounds produced by physical Powers ..... *ib.*
9 Table of the Density of Vapours and Gases ..... *ib.*
10 On the Existence and Nature of Bromine 233
11 Preparation of Chloride of Boron 234
12 Chloride of Boron ..... *ib.*
13 Preparation of Chloride of Arsenic 234
14 Preparation of Oxyde of Chrome, as a Pigment 235
15 Separation of Iron from Manganese ..... *ib.*
16 Pure Proto-Salt of Iron 236
17 Action of Potassium on Oxalates *ib.*
18 Analysis of the Triple Prussiate of Potash ..... *ib.*
19 Preparation of Medicinal Chloride of Soda..... *ib.*
20 Mode of Action of Disinfecting Chlorides..... 237
21 Deleterious Effects of Carbonic Gas 238
22 New Acids obtained from Animal Substances by Alkalies 239
23 Sugar of Melons ..... *ib.*
24 Method of Destroying the Empyreumatic Odour of Alcohol .. *ib.*
55 On the Colouring Matter of the Madder..... *ib.*

III. Natural History.

1 Subterraneous Noises heard at Nakous 240
2 Italian Cavern containing Bones 241
3 Crystallization of Asbestus .... *ib.*
4 Dense Fogs ..... *ib.*
5 Atmospheric Phenomenon..... 242
6 Effects of Cold ..... *ib.*
7 Notice of Mr. Burchell's Travels in South America ..... *ib.*
8 Note on the Motions and Actions of Quadrupeds 243
9 Measurements and Weight of a Sea-gull 244
10 Excision of Teeth ..... *ib.*
11 Poisoned Wounds 245
12 Bites of venomous Reptiles.... 246
13 Poison of Rattlesnakes..... *ib.*
14 On Two new Kinds of Urinary Gravel ..... *ib.*
15 Conquest of Peru and Mexico, by the Mongols, in the Thirteenth Century 247

Meteorological Diary 248

No. II.

Page

On the supply of Water to the Western Parts of the Metropolis . 249

On the Cultivation and Improvement of Edible Fruits . . 265

"The Gold-Headed Cane," *reviewed* 278

Revised Arrangement of the Ornithological System of Cuvier and Dumeril, with reference only to the Genera and Species of the British Islands . . . . . .

Dr. Mac Culloch on Malaria 294

"Transactions of the Horticultural Society of London, Vol VI. Part 5," *reviewed* 317

On the Effect of certain Exercises on the Form, and their Influence in preventing or curing Distortions of the Spine . . . 319

On the Introduction of Salt as an Agricultural Agent . . . 337

Description of the Kaleidophone, or Phonic Kaleidoscope; a new philosophical Toy, for the illustration of several Interesting and Amusing Acoustical and Optical Phenomena . . 344

London in 1826, with respect to its present state of Architectural Advancement 352

"De Plantarum, præsertim Cryptogamicarum, Transitu et Analogiâ, Commentatio," *reviewed* 362

On the Chemical Composition of two Liquids lately proposed as Disinfectants of great power. By Dr. Granville, F. R. S. &c. 371

Letter from Captain Sabine to Professor Renwick, on the Weights and Measures of New York 382

Further Observations on the Use of the Gelatin of Bones as an Article of Food. By Charles de Gimbernat 386

"Atlas Ethnographique du Globe, ou Classification des Peuples Anciens et Modernes, d'après leur Langues, par Adrien Balbi," *reviewed* 329

Hieroglyphical Fragments 402

On Achromatic Microscopes, with a description of certain Objects for trying their defining and penetrating Power. By Dr. Goring. 410

Astronomical and Nautical Collections;

i. Corrected method of computing an observed Occultation . 434

ii. Remarks on the Solar Tables. By Thomas Henderson, Esq. 438

iii. Answer to some Remarks of Mr. Ivory 439

iv. Elementary View of the Undulatory Theory of Light. By Mr. Fresnel 441

Proceedings of the Royal Society 455

Proceedings of the Royal Institution of Great Britain . . . 456

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1 On the compression of Gases.. Page. 467
2 On a New Optical Property of Dichroite 468
3 Velocity of Cannon Balls .... Page. 468
4 Security from Fire in Theatres ib.
5 Macadamized Roads..... 470

II. Chemical Science.

1 Determination of the Electro-conducting power of Metals .. 470
2 On Lightning-rods and Compasses 471
3 On the Transference of Ponderable Matter in Electric Discharges 472
4 New Phenomena produced by Vapour ..... ib.
5 Transference of Heat by change of capacity in Gas..... 474
6 On the change of Zero in Thermometers 475
7 On Bromine and certain of its Compounds 476
8 On Oxide of Iodine and Iodous Acid 477
9 Quantity of Charcoal obtained from Wood 479
10 Preparation of Cyanide of Iodine 480
11 On the Detection of Hydrocyanic Acid in the Bodies of Animals poisoned by it ..... ib.
12 On the presence of Ammonia in the natural Oxide of Iron.... 481
13 Volcanic Muriate of Ammonia of the Isle of Lanzerole .... ib.
14 On the Efflorescent Salts of Volcanic Rocks..... 482
15 On the Nitrates of Ammonia and Mercury ..... ib.
16 Peroxide of Potash from Nitre 483
17 Crystallized Carbonate of Potash ..... ib.
18 Sulphates of Manganese and Soda ..... ib.
19 Composition of White Copper, or Packfong ..... ib.
20 Method of Bronzing Tin..... ib.
21 Chromate of Silver crystallized 484
22 Sulphate of Quinia ..... ib.
23 Adulteration of Sulphate of Quinia with Sugar ..... ib.
24 Composition of Vegetable proximate Principles 485
25 On Legumine ..... ib.
26 Substances contained in Essential Oils..... 486
27 Change of Oil of Turpentine by exposure to Air..... 487
28 Preparation of Cafeine 488
29 Black Dye and Ink prepared with Logwood ..... ib.
30 Improvement in Varnishes .... ib.
31 Substances obtained from Castor Oil ..... ib.
32 Distillation of Fatty Bodies .. 489
33 Action of Nitric Acid on Hogs' Lard ..... ib.
34 Ozmazome 490

III. Natural History.

1 On Death by Insufflation..... 491
2 Remarks on the Human Voice ib.
3 Sabulous formation in the Brain 492
4 Analysis of a Ferruginous Renal Calculus..... ib.
5 Production and Preparation of Pearls 493
6 Peculiar Cases of the Use of Milk as Food..... ib.
7 Preparations of Henbane and Belladonna..... ib.
8 On the Culture of Bees in Forests 494
9 Signs of Increase, Maturity, and Decay in Trees ..... ib.
10 Structure of Plants..... 496
11 Experiments on the Fecundation of Vegetables..... 497
12 On a natural Botanical Arrangement dependent on chemical characters 498
13 On the Application of a Ligature to Fruit-trees and to Vines 499
14 Method of reviving Plants, Shoots, &c. ..... ib.
15 Ancient Glass Bottles..... 500
16 Natural Prognostics of the Weather..... 501
17 On Ancient Aërolites..... 502
18 Fresh Water found at Sea . .. ib.

TO OUR READERS AND CORRESPONDENTS.

THE pages of this Journal are impartially open to all communications upon the subjects of Science, Scientific Literature, and the Arts; and it is requested that they may be forwarded to the Editor, at least one month previous to the period of the publication of each Number.

We shall be happy to receive papers from Provincial Scientific and Literary Societies, and to publish them either on the part of the Society, or of their respective authors.

Papers which are too long for insertion, as is the case with many which we receive, or which are deemed unfit for this publication, will be immediately returned to the source whence we receive them, with our reasons for their return.

Three letters have reached us upon the subject of the Grand Junction Water Company's supplies. We quite agree with the writers as to the state of the liquid furnished, but have had no time to inquire into its cause. It certainly has at times "a most antient and fish-like smell."—We shall notice the *Dolphin* in our next Number, recommending it, in the mean time, to the serious consideration of those whom it concerns.

Mr. Heineken's paper, from Madeira, reached us too late for insertion, but an abstract of its contents will be given in our next Number. We cannot take upon ourselves to transfer it to any other Journal without the Author's special directions upon the subject.

PHILOCHEMICUS has come to hand, but we cannot persuade ourselves to admit a mere chain of argument as evidence against experiment. We allow the insufficiency of the views which he combats; but nevertheless the experiments upon which they are founded are stated—if Philochemicus will repeat these, and show that the author is mistaken in his conclusions, we shall immediately insert his communication with thanks.

TO READERS AND CORRESPONDENTS.

Communications have been received from Dr. Mac Culloch, from the Medico-Botanical Society, from Mr. Rumbal, Mr. Swainson, Dr. Goring, and several anonymous Correspondents, which, either from the lateness of their arrival, or from want of space, we have been under the necessity of postponing.

It is quite out of our power to comply with the wishes of many of our Correspondents for extra copies of their papers. Were all their requests acceded to, it would be impossible that this Journal could be printed and published at the appointed time, and we obviously can make no distinction. We therefore trust that our refusal will be considered as a matter of necessity, and not arising out of want of courtesy or consideration.

All manuscripts sent us for publication are invariably destroyed as soon as the number in which they are printed is published, except we receive a special request that they may be returned to the authors, which is always attended to.

We are much obliged by the trouble taken by our Correspondent, N. R. D. The pump is certainly not new, and probably not good for much, or it would have come into more general use. The other suggestions are not forgotten.

A letter has reached us, signed "An Amateur." We trust that the present Number will meet his views.

The communication upon the subject of the recent *discussions* in the Royal Society is omitted, for reasons which we presume will be sufficiently obvious to the communicator; but the subject shall not be lost sight of.

Works in the Press.

Some Account of the Science of Botany; being the Substance of an Introductory Lecture delivered in the Theatre of the Royal Institution of Great Britain, by John Frost, F. A. S. and L.S., of Emmanuel College, Cambridge, &c. Dedicated, by permission, to the King.

Malaria: an Essay on the Production and Propagation of this Poison, and on the Nature and Localities of the Places by which it is produced; with an Enumeration of the Diseases caused by it, and of the Means of preventing or diminishing them, both at Home and in the Naval and Military Service. From the pen of Dr. Mac Culloch.

THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

Sketch of the Life of the late Samuel Parkes, Esq.

Mr. Editor, *Burton Crescent, Feb.* 26, 1827.

Thinking that a short sketch of the life of a man so generally known and deservedly esteemed, and whose chemical works had become so universally popular, as those of the late Mr. Samuel Parkes, would not be uninteresting to the readers of your valuable publication, I have examined his diaries, and endeavoured to compress within as small a compass as possible, those facts and circumstances which appear likely to afford the most correct delineation of his character.

I remain, your's faithfully,

J. W. Hodgetts.

Mr. Parkes was the eldest son of Mr. Samuel Parkes, of Stourbridge, in Worcestershire, where he was born on the 26th of May, 1761. At the age of ten he was sent to a classical school, kept by Dr. Addington, of Market Harborough, in Leicestershire. For this gentleman, whom he describes as having possessed "considerable oratorical talents," "and great attainments, classical and literary," Mr. Parkes always expressed strong feelings of respect, and acknowledged that, to his instructions and care, he was indebted for that love of literature, and those habits of application which never deserted him in after-life.

He continued at this school four years, when he was taken away in order to be apprenticed to an ironmonger at Ross, in Herefordshire; in consequence of whose failure he

returned home, and remained in his father's business, that of a grocer, till he settled for himself.

During this period of eighteen years, he appears to have devoted himself assiduously to literary pursuits, and to have spent much time in perusing standard works upon almost every subject necessary to the completion of a liberal education. He neglected no opportunity of collecting portraits for his illustrations of Grainger, or of purchasing scarce and valuable books; indeed he seems then to have commenced those collections which he afterwards brought to such perfection. His diaries contain a minute detail of each day's employment, and a list of the books which he was reading; and in common-place books, of the same dates, are inserted his opinions of the contents, and transcriptions of every passage which he deemed worthy of preservation. I can find no diary preceding 1783; but as others are referred to, am inclined to believe that he commenced writing them at a considerably earlier period.

In 1786, he seems to have given up a good deal of time in concert with Mr. Isaac Hawkins Browne, late member for Bridgnorth, in devising measures for the suppression of counterfeit halfpence throughout the kingdom; and soon afterwards I find, from the Earl of Stamford's letter, accepting the office of patron, that, after great labour, he succeeded in establishing a public library at Stourbridge. About this time, too, he wrote many papers for the Gentleman's Magazine, chiefly upon subjects of Theology, which was a favourite topic with him in early life, and occupied a considerable portion of his attention.

The large intervals of leisure which he enjoyed during the time of his residence under his father's roof, being devoted exclusively to the acquisition of knowledge, laid the solid foundation of those attainments, which in after-life rendered his name so eminent.

In 1793, he settled at Stoke-upon-Trent, as a soap-maker, and from this time he dated the commencement of his chemical pursuits. He soon succeeded in obtaining his alkali from the decomposition of the sulphates in a way very superior to any which had previously been adopted, and introduced great im-

provements into every branch of the manufacture. In 1794, he married Miss Twamley, of Dudley, and remained at Stoke, pursuing earnestly the study of chemistry, till 1802, when, wishing to make his only child a proficient in his favourite pursuit, and unable to find an elementary work calculated to teach the rudiments of the science, he began to compile, with great assiduity and care, what he thought better adapted for a young person than any treatise which he had seen. Such was the origin of the Chemical Catechism, its author not having at that time the slightest view towards publication.

Being, from unforeseen events, unnecessary here to detail, unfortunate at Stoke, where he paid but a small dividend to his creditors, he came to London in 1803, and commenced business as a manufacturing chemist, as he frequently used to relate, with 30*l.* of his own, and 100*l.* borrowed of a friend; and with this trifling capital was so successful, that, within three years, he was enabled to obey the dictates of his honourable feelings, by remitting to his creditors the amounts of their debts, with interest from the time they had been due. This business he continued during the remainder of his life; and his rapid and uniform success may, perhaps, be in great measure ascribed to the very accurate knowledge which he acquired of almost every manufacture which is at all dependant upon chemistry, and consequently of the precise nature of those substances which might be beneficially employed. Soon after his arrival, a much-esteemed friend, wishing to instruct his children in chemistry, and not approving of any work which he had been able to procure, applied to Mr. Parkes for advice, who told him that he had himself been in a similar difficulty, but willingly lent him the manuscript which he had written to obviate it.

Pleased with the perusal, and convinced that it was exactly the book that had so long been required, he became urgent in his importunities with Mr. Parkes to publish it. At length, though opposed by the fears and objections of his family, and notwithstanding his retiring habits, so averse to appearing before the public, he succeeded in persuading him. The work was carefully revised, the notes, tables, experiments, and index added; and, indeed, whatever indefatigable assiduity

could attain to render it at once amusing and instructive, was done. This edition of 1500 copies (the smallest that he ever printed) was published in May, 1806, and sold so rapidly as to make a second necessary in October, 1807. From that period to the present, the demand has been incessant, eleven editions having been called for within eighteen years; none of them were mere reprints, Mr. Parkes having always materially altered and enlarged them, and indeed several were almost rewritten. The sale abroad has kept pace with that at home, and copies have been transmitted to the author of editions published in several of the States of North America, in various parts of Germany, in France, Spain, and Russia.

Perceiving how much a still more elementary work was required, particularly by schools and young persons, which might by its bulk and price be easily accessible to all classes of readers, he wrote the Rudiments of Chemistry, in which the arrangement of the Catechism is preserved, and indeed the whole of the substance of the text, the catechetical form being, for the sake of brevity, dispensed with, as are also the notes and tables; the principal facts and axioms are printed in a large type, and under each, in a smaller letter, the experiments and illustrations necessary to elucidate them. He published the first edition in 1809, and so much was a book of this nature wanted, and with such avidity was it received, that, in the autumn of 1825, a fourth edition was called for; besides which, it has been reprinted in New York and Philadelphia.

His other principal work is the Chemical Essays, which were written in furtherance of the great object of which he never lost sight, viz., the application of chemical science to the arts and manufactures of the country; they have passed through two editions, and have been republished in France and elsewhere. The essays are on various subjects, and contain detailed accounts of all the processes employed in many of those manufactures which are most dependent on a correct knowledge of the principle of chemistry: he takes every opportunity of suggesting improvements in the different methods of procedure, and had the satisfaction of hearing that many manufacturers had profited by his suggestions, the adop-

tion of which had been attended with the most successful and beneficial results.

Besides these standard works, he was a frequent contributor of papers to the different periodical scientific publications, and to the numerous societies of which he was a member. The readers of this Journal must be familiar with his articles, as it is a work for which he always professed the highest regard; and perhaps the Royal Institution possessed no member more warmly attached to its interests, or a more zealous supporter of the objects for which it was established.

The Highland Society of Scotland presented him with a silver inkstand for his essay "On the comparative value of Kelp and Barilla, describing the component parts of each, and the various uses to which they may be advantageously applied." The Caledonian Horticultural Society also awarded him a silver cup, in token of their approbation of his "Essay on the use of Salt in Horticulture."

In June, 1808, he was examined before the West Indian Committee of the House of Commons, relative to the uses of sugar in fattening cattle; but more particularly on the prevention of frauds upon the revenue, by the mixture of different substances with it, to hinder it from being used for domestic purposes, or for distillation, but at the same time without injuring its nutritive qualities. His evidence, detailing at length his experiments and opinions on this subject, was afterwards published in the Philosophical Magazine: the paper contained much very interesting and novel matter, and gained him great credit with the public.

The investigation of the various uses of salt, and his anxious wish for the abolition of the salt duties, were objects of his most persevering and unremitting attention for a long series of years. In 1817, he first turned the notice of the public seriously to the subject, by the publication of his "Thoughts on the Laws relating to Salt." In this work he describes the numerous and important uses of salt in agriculture, in feeding cattle, and in the manufactures of muriatic acid, oxymuriatic acid, soap, and soda, with the great advantages which would be derived if it were lawful to fabricate barilla and alkali by the decomposition of sea-salt, without payment of duty: he

enumerates the divers other manufactures which would be created by the repeal of those laws, and proceeds in a forcible manner to show the baneful effects which they have had upon those great nurseries of our seamen, the fisheries of Great Britain. In February, 1819, he first published his "Letter to Farmers and Graziers on the use of Salt in Agriculture," in which he details the methods of its advantageous application as a manure, in feeding cattle, in preserving hay, in the destruction of worms and insects, and even in rearing bees; he also inserts copious extracts from the evidence given on the subject of the Salt Laws before the Board of Trade, in April, 1817, and before a Select Committee of the House of Commons, in April, 1818, on both of which occasions he was examined at great length for many days; and the united testimony of the most celebrated agriculturists and men of science of whom England could boast, went to prove the inestimable advantages that would accrue to the country from the repeal of those laws.

The interest that was excited on the subject by this book, and the consequent rapidity of its sale, were probably unprecedented. The first edition, published on the 22nd of February, sold off instantaneously; the second came out in March, the third on the 1st of May, and the fourth on the 1st of November of the same year. The late Sir Thomas Bernard was a fellow-labourer with him throughout this question, and gave his time and attention assiduously to it. At length, after twenty-eight years of unwearied exertion, during a considerable portion of which period little hope had appeared of ultimately obtaining any beneficial result, Mr. Parkes had the satisfaction of seeing his labours crowned with success by the total repeal of the laws in 1825, since which time the increased consumption of salt has fully justified his statements, and proved the correctness of his anticipations.

Engaged, as Mr. Parkes was, in the management and superintendance of an extensive chemical manufactory, which required unremitting attention and the greatest portion of his time, his works were composed at those hours which would generally be devoted to amusement or repose; but such was the unwearied and indefatigable nature of his mind, that,

besides his books, which demanded his constant care in preparing for new editions, in altering and adapting them to the perpetually-improving state of the science, and his papers for the periodical publications, which were all composed with more than ordinary pains, (as, for instance, the last paper which he ever wrote—"On the analysis of some Roman Coins," published in the Quarterly Journal for July, 1826, during the composition of which he submitted to accurate analysis upwards of twenty coins, costing him the incessant labour of many months, as he made it an invariable rule never to place dependance upon any experiment till it had been repeated, and the same result again obtained)—besides all these avocations, which appear amply sufficient to fill up the time of one man, he kept a regular diary of every action of his life, and from an early age till within a few months of his death, never once retired to rest until he had committed to writing all the events of the day. He had common-place books, in which he entered everything worthy of note, whether gathered from reading, observation, or conversation, and he never performed a single chemical experiment without registering every particular of the process and the result. By these means he had always a mass of information collected upon most scientific subjects, and no improvement or valuable fact, once known, could ever be lost or forgotten, but, if it related particularly to chemistry, was sure, if useful, to appear in the next edition of the Catechism. As if all these occupations were insufficient to give full employment to his hours, his beautiful collections of Greek and Roman coins, prints, autographs, minerals, and fossils, were constantly and rapidly increasing, and, as fast as collected, were arranged in the most methodical manner.

On several important trials, Mr. Parkes was a very material witness; and where the question at issue hinged on a chemical point, as was so peculiarly the case in that between Messrs. Severn, King, and Co. and the Insurance Offices, he never hesitated to give up his time and attention for weeks and months in studying the subject, and in trying the requisite experiments with precision and accuracy. In the year 1818, Mr. Parkes was applied to by Messrs. Turner, Woodhead, and Co., of Rotherham, upon whom seizures had been made by

the Board of Excise of British gum which had been prepared in their starch-manufactory, upon the pretence of its being brown starch that had not paid duty; the penalties sued for amounted to upwards of 3000*l*. Samples were sent to him for analysis, on a careful examination of which he found that they possessed none of the properties of starch; and when the cause came on to be tried in the Court of Exchequer, in which he was subpœnaed as an evidence for the defendants, having obtained permission to repeat in court the experiments by which he had ascertained the points in which British gum differs from starch, the proofs were considered to be so decisive, especially the one of the beautiful blue precipitate produced by the solution of iodine, that a verdict was immediately pronounced for the defendants.

It is truly wonderful that one man should be capable of doing all that he did; that, actively engaged in business as he was for so many years, he should be able to write so much, and yet be always prepared to follow with ardour any scientific investigation, or chemical analysis, that might present itself. In experiments he was delicate and sure, never disheartened by failure or by the tediousness of the operations, and rarely stated any fact in his books till he had previously verified it himself.

The Chemical Catechism was the work to which its author was the most fondly devoted; it was his first production; it was the foundation on which his reputation as a chemist had been raised: twenty years had not lessened the attachment of the public to it, and the unusually large editions which he published were called for with even more rapidity at last than they had been at first. The Rudiments of Chemistry, and the Chemical Essays, had both been received with unequivocal marks of approval,—each had passed through large editions, and been quickly sold; the essays had been translated into various languages, and both had been republished in different parts of the United States: but the Catechism was, as it were, identified with his very name, it bore all the characteristics of his powerful and original mind; to bring it nearer to perfection had been his most assiduous labour, his most cherished pleasure, for twenty years. The perusal of the Cate-

chism had induced the principal literary societies of both hemispheres to present him with their diplomas, and to elect him one of their members: the Government of Spain had ordered it to be used in the schools and colleges; and, in testimony of his approbation of it, the Autocrat of the Russias presented its author with a splendid and valuable ring.

The preparation of the twelfth edition for the press was the last avocation of its author. Although his body had for months been fast sinking beneath the constant attacks of a long and painful disease, which the foreboding fears of his friends too truly predicted must at length terminate fatally, yet his mind, unwearied and indefatigable as ever, preserving in full force its characteristic activity and diligence, was sedulously employed in the revision and improvement of its favourite production. The same anxiety to collect and insert all newly-discovered facts, all recent improvements in the science to which he had so long devoted his time and energies; the same ardour of research, and attentive examination of every source whence he expected to derive useful information, were possessed by him during the performance of this last work as they ever had been whilst in the full enjoyment of bodily health, nay, existed in full force till within a few days of that fatal moment when the scientific world was for ever deprived of his talents and acquirements, and his friends were left to deplore the loss of an affectionate relative, a sincere friend, and interesting companion.

For several years preceding his death he had been collecting and arranging materials for a work, the composition of which he had long contemplated. He entitled it, "A History of the Discoveries in the several branches of Natural Philosophy, Astronomy, and Chemistry; including an account of the introduction of the various kinds of chemical apparatus, and the invention of all philosophical, nautical, and astronomical instruments, from the beginning of the Christian æra to the present time, affording a general view of the progress of Science and Literature during that period." It is to be regretted that he did not live to complete it, as such a subject in his hands must have proved a valuable acquisition to those who delight in tracing the gradual but regular advances of knowledge.

Many other essays, on different subjects, which he never published, attest the varied nature of his acquirements.

In June, 1825, Mr. Parkes, whose health had been for some months past slowly declining, was taken ill at Edinburgh, and brought to London by easy stages. All that could be done by the most eminent physicians was done; but the disease was too deeply rooted to be eradicated, and his family had the pain of watching him fast sinking away from his sphere of usefulness. Throughout the whole of his acute and severe sufferings, he was never heard to repine, but always expressed the most lively pleasure at receiving the visits of his numerous friends; and the last sentence he uttered was to thank a gentleman for his kindness in coming to see him. His friends, however, only alleviated his pain, and after a severe struggle he closed his active life on the 23rd of December, 1825.

Mr. Parkes was a man of an inquiring and unwearied mind, persevering and ardent in the pursuit of knowledge, and happy in his mode of imparting it to others. Not a moment of his day was ever suffered to pass unemployed: after writing upon one subject for a long series of hours he retired from his study, not from the slightest feeling of fatigue, but because the time for his meals, or for rest, was arrived. Whatever subject he was perusing, he never abandoned it till he had fully accomplished his object, and till every source of information was exhausted. By turns he had attended to almost every department of literature, and of general science, indeed was well versed in them; but natural philosophy, and more particularly the chemical branch of it, was his favourite pursuit, and engrossed most of his attention. His well-stored library afforded him every facility of reference, and he bestowed much pains on its selection and arrangement.

Probably no author ever attracted so many students to the science on which he was treating, as Mr. Parkes did to the study of chemistry by the publication of the catechism: the easy familiarity of its style, the happy knack which he possessed of rendering the driest and otherwise most unattractive parts inviting and pleasing, by interspersing them with useful and interesting matter not immediately connected with his subject, added to the admirable system of arrangement, adapted

to lead imperceptibly from the rudiments to the higher branches of the science, all conduced to allure the reader to pursue with ardour so fascinating a research.

Few persons have been engaged in so extensive a literary correspondence, both in Europe and America. He courted the friendship and conversation of scientific men both at home and abroad, considering them as some of the most valuable sources of knowledge; to him they were sources easily attained, as his works had rendered his name familiar to an unusually large circle.

In private life, he was benevolent, affectionate, and sincere, warm and constant in his attachments, rarely offended, and easily and quickly conciliated. To his servants he was an indulgent and generous master, a contributor to, and supporter of, every institution established for the purposes of charity, education, or the promotion of knowledge. Few men have ever departed this life whose loss will be felt in so wide and varied a circle as Mr. Samuel Parkes; but that loss can only be duly appreciated by those who possessed the gratifying privilege of calling him by the name of friend.

On the recent Adjudgment of the Royal Medals, by the President and Council of the Royal Society.

Mr. Editor,

The destination of the Royal Medals, as lately determined upon by the President and Council of the Royal Society, has excited no small degree of surprise and dissatisfaction in the minds of many of the Fellows, and of a large portion of the scientific public; and I am induced to request you will insert this letter in the Quarterly Journal, lest silence on the part of the malcontents be construed into acquiescence, and considered as justifying the singular proceedings of the Council, proceedings which, in my mind, are calculated to injure the cause of science, to blunt emulation, dishearten activity, and rob zeal and diligence of their fair prospects of success.

I must, in the first place, disclaim all personality towards

Mr. Dalton, with whom I am utterly unacquainted. I am told that some of his meteorological papers rise above mediocrity, and that he is reserved and sagacious; that his mind is stored with important knowledge, which, either through indolence or modesty, or both, he is prevented from producing to the world. But these are matters little relevant to our present discussion. —His *New System of Chemical Philosophy* I have read and studied; upon its disclosures we find his present claims are founded, and to it we must refer as the standard of his originality in discovery, and of his title to the high distinction which has just been conferred upon him. I say *high distinction*, because, in the present case, there is something more than the mere reward of an honorary medal. It is a medal of Royal foundation; it is now presented for the first time; and a kind of precedent is established, upon which future Presidents and Councils may act. It is, moreover, a distinction conferred, not by a Society of to-day's creation, nor by an assembly of artisans, nor by a school of mechanics, but by one of the most ancient and respectable scientific bodies of Europe; so that the fame thereof will go abroad, and will be sounded even in the remotest corners of the earth. These are circumstances which should have been well weighed and cautiously considered, by the bestowing parties; and, instead of obsequiously adopting, as I fear they have, the biassed suggestions of some one or two prejudiced individuals, they should have met the case with manly and open independence, and, shaking off all the prejudice of party, and all the petty jealousies of rivalry, should have acted with fearless integrity up to the motto:— "*Palmam qui meruit, ferat.*"

I read, in your last Journal, "that the Council awarded the first prize to Mr. John Dalton, of Manchester, for the development of the chemical theory of definite proportions, usually called the Atomic Theory," and the President afterwards alludes to Higgins, Richter, Wollaston, Gay-Lussac, Prout, and some others, of minor notoriety, as labourers in that vineyard, of which Mr. Dalton is recognised as lord and master.

From the above statement, however, so little is to be gathered, that I must endeavour to form some more exact scale by which I may safely measure his pretensions, and satisfac-

torily determine their weight and magnitude; and for this purpose it will be necessary to solve at least three questions:—1. Is Mr. Dalton the inventor of the Atomic Theory—is he the discoverer of those peculiar laws of combination and decomposition upon which it immediately rests?—2. Has Mr. Dalton essentially contributed to its development—was it useless and barren in the hands of its inventor, and was he the first to promulgate its importance as bearing upon the general laws of chemical science—did he first show its practical applications, and render it available to the manufacturer and to the artisan?—3. Has he disclosed any new views connected with the applications of the theory to the more abstract branches of chemical science?

Of these questions, the first is the most essential to Mr. Dalton's claims; but it unfortunately happens that they stand negatived upon the most direct of all evidence, and that Mr. Higgins is decidedly the discoverer of those principles of combination upon which one part of the atomic theory is founded. Upon him the grave has closed, and he is no longer able to defend his personal pretensions. Had he been alive, I much doubt whether sufficient effrontery could have been mustered to venture upon the step I am discussing. Let us now look a little into the matter, and listen first to the President himself.—

"That the proportions in compound gases are definite, has long been generally acknowledged; but Mr. Higgins is, I believe, the first person who conceived that, *when gases combined in more than one proportion, all the proportions of the same element were equal;* and he founded this idea, which was made public in 1789, on the corpuscular hypothesis *that bodies combine particle with particle, or one with two, or three, or a greater number of particles.*" Sir Humphry then adds, "Mr. Dalton, about 1802, adopting a similar hypothesis, *apparently* without the knowledge of what Mr. Higgins had written, *extended* his views (Mr. Higgins's) to compounds in general*."

Let us now hear Mr. Higgins†: "These considerations,"

* Elements of Chemical Philosophy, p. 107.

† Experiments and Observations on the Atomic Theory and Electrical Phenomena.

(adverting to the views just quoted from Davy) "gave birth to that doctrine which Mr. Dalton, eighteen years after I had written, claimed as originating from his own inventive genius. What his pretensions are will be seen from the sketches which will soon follow, and which have been taken from my book.

"In the application of my system to chemical theory, I expressed by numbers the relative forces of attraction subsisting between the different kinds of ultimate particles and atoms of matter to each other. This was of infinite use to me during my researches, and, if followed up, would mature the science into that mathematical precision in which it is at present deficient.

"This I consider as one of the most important features of the system; it has not even been mentioned by Mr. Dalton in his work, and when he repeats experiments formerly made by me, he does not even glance at the source from whence he derived his information.

"I cannot with propriety or delicacy directly say that Mr. Dalton is a plagiarist, although appearances are against him. Probably he never read my book; yet it appears extraordinary that a person of Mr. Dalton's industry and learning should neglect one of the few works that were expressly written on the subject of theory."

Lastly, Dr. Ure observes*: "Is there any law regulating these various compounds (of nitrogen), so that, knowing the first proportion, we may infer the whole series? This question was first considered in a work containing many curious anticipations of discoveries to which posterior writers have laid claim. I mean Mr. Higgins's comparative view of the phlogistic and antiphlogistic theory, printed in 1788, and published early in 1789. Besides some additional facts decidedly hostile to the hypothesis of phlogiston, this publication indicates the doctrine of multiple proportion, with regard to the successive compounds of the same constituents. This was likewise interwoven with new and ingenious views concerning gaseous and atomical combination. Mr. Higgins, having felt himself aggrieved at seeing discoveries first announced by him in 1789,

* Dictionary—Art. "Equivalents."

brought forward nineteen years afterwards by Mr. Dalton in his own name, published, in 1814, a book, entitled *Experiments and Observations on the Atomic Theory and Electrical Phenomena.* In this work he gives numerous quotations from his *Comparative View*, which appear to establish his claim of *priority to the discovery of multiple proportions and the atomic theory of chemistry.*" Dr. Ure then proceeds to quote and explain Mr. Higgins's experiments, and leaves not the smallest doubt, upon a candid and unprejudiced mind, of his claims of priority and originality.

I now presume to have shown that Mr. Dalton is not the inventor of the atomic theory, nor the discoverer of those laws of combination and decomposition upon which it is immediately founded. I next ask, whether Mr. Dalton has materially contributed to its development and extended application? Whether we owe to his suggestions those "Tables of Equivalents," which are so useful in the laboratory, and so important to the manufacturer of chemical products?—No: all this is due to Dr. Wollaston, whose *logometric scale of chemical equivalents* brought the theory into practice, and rendered that, which was a mere abstract subject of chemical inquiry, little understood and less investigated—an instrument of the utmost usefulness and value. He did that for the theory of definite proportions which Mr. Watt effected for the steam-engine: it was a comparatively useless and cumbersome machine; he rendered it generally applicable, and divested it of its clogs and incongruities. Had Mr. Dalton stood in the place of Dr. Wollaston, I should have gladly witnessed the honour which has fallen upon him; for the merit of those who fertilise barren inventions, and bring them home "to the business and bosoms of men," I am inclined to rate very highly, and even sometimes to consider it as superior to that of the original inventor. Newton developed the theory of gravitation, and magnificently did he apply it to practical purposes; but such excess of talent rarely falls to the human lot:—

> "For life doth rarely to one man allow
> Time to discover worlds and conquer too."

And it generally happens that a great axiom or principle in

science is unfolded by one mind, and applied and extended, as in the case before us, by others. The decomposing energies of the pile of Volta were discovered by Nicholson, Carlisle, and Cruickshank. They were extended and applied to chemical generalisations by the eminently superior talents of Davy. Mr. Tennant discovered that explosions would not pass small tubes; and this is the clue to the mystery of the safety-lamp. But every page of the history of science records analogous instances, and shows that, however eminent may be the merit of an inventor, he who applies and improves upon the invention, who extends its precincts, and levels it to the purposes of common life, is the real benefactor, and that he it is who should reap the praise and enjoy the renown; but such, as I have before said, is not Mr. Dalton.

My third query is directed to the applications of this theory to the more abstract and refined departments of chemical science; and happy should I be, if, under this head, I could find grounds of justifying the determination of the Council in regard to this royal medal; here it is that Richter comes in for his share of conquest, and that Gay-Lussac stands pre-eminent; but here chiefly it is that we ascribe, as is most due, much honour and credit to the laborious industry of Dr. Prout, who has applied the theory of proportions to some intricate questions of vegetable and animal composition, and who has thrown out some singularly felicitous hints on the relations between the specific gravities of bodies in their gaseous states, and the weights of their atoms.

Thus, then, I have endeavoured candidly to weigh Mr. Dalton's pretensions, as urged in the President's speech, and find them, in their utmost latitude, insufficient and unsatisfactory. Even his best friends and professed admirers must allow, if they have condescended to read the preceding quotations, that his claims are certainly not founded upon a rock, that at least much suspicion hangs over their authenticity; and if there be any who are not satisfied with the evidence I have adduced, let them attentively peruse "Higgins's Comparative View," and afterwards the second volume of "Dalton's New System," "and then to breakfast with what appetite they have." The fact is, that over the present question there should

neither hang doubt nor discrepancy; the title should be without flaws, the claim without a drawback; and that which crowns the impropriety of the present adjudgment is, that a list of names might easily be produced upon whom the distinction might have been conferred without a shadow of impropriety or inconsistency; three or four such may be found in the Council itself, and several in the lists of the Society: Philosophers, whose discoveries are really their own, and who have not only enlightened and enriched science, but have exalted the name and fame of the Royal Society by their splendid contributions to its published Transactions, a set of volumes in which the name of Dalton scarcely appears, except annexed to one or two unimportant and flimsy papers.

Of Mr. Ivory I have much less to say; he is universally admitted to be a deep and skilful mathematician, but his researches are, unfortunately, of a very abstract nature; and though of his abilities and originality there may be no doubt, the usefulness of his inquiries is open to a query. Some may find an interest in abstruse investigations, having for their object the determination of the figure which the earth would assume were it made of calf's-foot jelly instead of rock and stone, and such a question may cost months of daily and nightly labour to solve: much midnight oil, and much sweat of the mental brow may be expended in its determination: but when settled, where is the fruit of the demonstration?

Upon the whole, it may, I think, fairly be assumed that, although the claims of Mr. Ivory to the honour which has now been bestowed, are far more substantial than those of Mr. Dalton, they are yet open to objection, and that, upon the present important occasion, he whose discoveries and inventions have most largely contributed to the general extension of science, or most decidedly bear upon the progress and perfection of the useful arts, should have taken precedence, and should have been selected from among his less industrious compeers as most worthy in himself, and most efficient as an example to others.

I am, Sir, yours, &c.

F. R. S.

Essays and Gleanings on Naval Architecture and Nautical Economy.

The impulse which these branches of naval science have lately received, is well exemplified in the above periodical work. This work is worthy of particular notice at the present eventful period of the arts. Not only do the national prosperity and honour depend on our naval ascendancy, but even our safety and existence as a nation would be endangered by signal defeats at sea. It may be asserted, without fear of contradiction, that the whole range of nautical economy never demanded more serious and ardent attention from our government than at the present time. A vigorous branch of the British stock now actually rivals us in commerce, and has shown the native prowess of its descent by deeds in war, worthy of the original and experienced parent. The junction of France and Spain, uniting a long and contiguous coast of ports to this country, and the internal aspect of affairs in Britain, enforce on us the watchfulness of serious concern, if they do not sound the tocsin of enthusiastic exertion. So much is the present compound existence of the British nation engrafted on her command of the seas, that a single defeat, or a suspension for a short time of her domination, would be a most convulsive crisis, fraught with ruin and inexpressible distress.

The progress of events and new discoveries render alterations expedient; and it is only by constant examination of these circumstances, and by fostering a knowledge of the principles on which they depend, that we can determine on the line of conduct to be adopted. Repeated epochs have shown us, that the forms of our ships have frequently become antiquated and unfit for further use. Such were the lofty, broad, high-sterned vessels of the seventeenth century. Subsequently, the forty-four gun ships, on two decks, came into disuse. Then followed the sixty-fours; and the seventy-fours are fast going out of vogue. The same might be said of other descriptions of vessels, which there is no occasion to particularise. It is worthy of remark, that they were all rendered useless by the superiority, in sailing qualities, of the vessels of our antagonists, who adopted other descriptions of ships, which we were soon obliged to imitate.

As all those manœuvres which are performed in military struggles by the marching of troops, are effected in naval

encounters by the sailing and the evolutions of ships, it is evident that victory may greatly depend on the qualities and characters of our floating batteries. On land it is not to be expected that any advantage can be calculated on from inherent superior celerity of movement, as the agents in each case must be supposed to be possessed of the same physical powers; in such case, it must be skill and valour alone that can give the ascendant: but, at sea, not only is ability advantageous, but increased facility of execution may be possessed by the superior architecture and equipment of vessels. In the instance of preponderating force being possessed by the enemy, celerity of motion in the retreat becomes of augmented benefit; as it is also, when, from the inferiority of the enemy, pursuit must be resorted to.

A large ship of superior velocity has the power of making every ship of inferior force its easy prey. The same may be said of fast-sailing fleets, which ought always to have the ships composing them of equal powers of sailing; as the velocity of the whole must be regulated by that of the slowest, in order to insure her protection.

Our enlightened naval historian, Captain Brenton, vol. i. p. 40, of his work, says—"Whoever reads with attention the history of our naval actions in the East or West Indies, America, or the North Sea, will readily attribute the failures of Hughes, Rodney, Graves, Byron, and Parker, to the miserable state of our shipping." And further on, he observes—"The Dutch, Spanish, and Russian armaments of 1787, 1790, 1791, called forth men who applied themselves with much assiduity to the improvement of the marine; the suggestions of officers of experience were attended to; the best and most approved models were selected and built after; and the *Courageux, of seventy-four guns, taken from the French* as far back as 1761, was the *favourite* of the service: the Leviathan was as near a resemblance to her as the builder of Chatham dock-yard could produce; and in the actions of 28th and 29th of May, and 1st of June, 1794, under the command of that high-spirited nobleman, the late Lord Hugh Seymour, this ship was one of the *earliest* in action."

Captain Brenton attributes the glorious victory under Lord Howe, in a principal degree, to the fast sailing of the Queen Charlotte and Royal George, ships of *enlarged dimensions*, which brought the enemy to action, by cutting the line at the desired time before the others.

From the same work we extract the following remarks, as

further proof of the lamentable comparative state of our shipping, and our deficiency of knowledge in the principles of the art. "The Victory is one of the most perfect vessels, of her size, we ever had; but we have unfortunately failed in our attempts to produce one exactly similar to her. The Boyne was so intended, but, on being launched, was discovered to be *two* feet narrower on the quarter-deck, and found to sail *wretchedly*."

It is unnecessary further to point out our degrading inferiority. The cause of this is plain; the nautical arts and sciences have been less cultivated in this country than in any maritime nation in the world. By the wise encouragement of talent, foreign nations have produced men learned in the art, and distinguished by their scientific acquirements*. It is not, therefore, matter of surprise, that the nation at large, and the navy in particular, have been loud in their complaints on the subject. The evil was in consequence remedied in part, by following the foreign nations closely in their models, for which we have been indebted to the unrivalled intrepidity of our sailors, who have captured their vessels; but this uncertain expedient has necessarily been accompanied with detracting attendants: our enemies have in the mean time improved on their models; and thus, with all the expense of dispensing with badly-constructed vessels, we have constantly been inferior to foreigners in the qualities of our warlike navy.

This exasperating deficiency, which has been so often reiterated throughout the nation, excited the ardent attention of many public-spirited noblemen, who proposed the formation of a "Society for the Improvement of Naval Architecture;" accordingly, a public meeting was convened at the Crown and Anchor Tavern, on the 14th of April, 1791, at which the late Marquis of Hastings presided. The institution was unanimously established and supported by numerous noblemen and gentlemen; among whom were, His Royal Highness the Duke of Clarence, Earl Stanhope, Lord Mulgrave, Sir Joseph Banks, Sir Charles Middleton, and Dr. Hutton. They all agreed to a resolution—"That the theory and art of ship-building being objects of the first magnitude and importance to these kingdoms, and not so well understood in this country as matters of so much consequence deserve, a remedy

* When a bridge, or other national edifice, is contemplated, many hundred pounds are given for plan and estimates; but how contrary is the practice in ship-building!

for this radical deficiency merited the attention of every well-wisher to the true interests of Great Britain." It is with regret it must be remarked, that the society totally failed in their object, by wasting all their energies and funds, in attempting to investigate the laws of resistance. In taking this step, they injured considerably the interests of naval science in this country, as will be presently noticed.

The murmurs and complaints, which we thus see reverberating through the nation, at length were made the subject of parliamentary interference. From the legislative authorities it echoed to the throne; and finally, by order of council, the proper remedy was proposed, and carried into effect, of expressly devoting young men of distinguished talent to the professional art of ship-building. The country at large, by public advertisement, detailing the most flattering prospect, and encouraging treatment, was invited to send its youth of education and ability to an open competition, in which the successful candidates were to be elected, and further promoted, solely from their merit. This important measure, which had long been pointed out to us for imitation, by the French and Spanish governments, with whom it had been so successful, was definitively regulated by the Board of Naval Revision in 1808. A school of naval architecture was therefore attached to the Naval College, Portsmouth; in which the students, chosen as above mentioned, are initiated in the elements and practice of their profession. As their attainments are carried to a wide extent in the mathematics, their capabilities are enlarged to a full and competent examination of their professional duties.

The work under consideration is the production of a few of these gentlemen. Its object, as expressed in its introductory pages, is "utility." It not only comprises original matter, but invites communications from all conversant with naval matters; and it leaves its pages open for the reception and collection of the vast quantity of valuable information published in foreign works. Thus, while it diffuses the principles of the art, it excites a spirit of inquiry. It was thought preferable to publish its matter in small and frequent portions, rather than in large numbers, for the purpose of admitting more extensive circulation, and communicating earlier intelligence of passing occurrences in the art. We are sorry to observe that there is now a suspension of these interesting labours.

Another periodical work was commenced on the same day, January 1, 1826, entitled, *Papers on Naval Architecture*. It appeared, however, that it was conducted on exclusive principles, several papers having been refused insertion in it, though coming from persons of undisputed attainments in the art; it was, therefore, judged expedient and conducive to the best interests of the science, to continue the Essays and Gleanings, as it was founded on a liberal and fair basis. Accordingly, the Essays and Gleanings made its appearance every subsequent fortnight, offering to its contributors a more convenient and beneficial opportunity of adding information to the general stock. We are happy to be able to say, that much good has been done by both publications; and, while we see emulation thus produced, we cannot but think it highly beneficial.

One peculiar advantage of the Essays and Gleanings over the Papers on Naval Architecture, consists, in the former taking a wider scope,—in the admission of practical, as well as theoretical knowledge—the actual construction of the machine, as well as the design of it. By this means, able practitioners have contributed their valuable observations in the art; among which may be particularly mentioned several interesting communications from Messrs. Harking and Blake, who are superior officers in His Majesty's dock-yards.

The *Papers on Naval Architecture* are almost wholly confined to the enunciation of the theorems that have been taught at the School of Naval Architecture, from the works of Atwood and Chapman. It must be confessed, however, that while all the problems so taught are proper to be known, they form but a feeble instrument in guiding the designer of ships. His information must proceed from an enlarged view of ships used for war on the ocean; he must be acquainted with the mutual relations of the different parts of a ship, and the relative proportions of the various classes, in order to systematise his ideas, and form a true theory from practice—for, in this case, theory must be practice reduced to system. It is on this account, that while we are glad to see Dr. Inman's translation of Chapman, with some valuable notes and new theorems, we regret that the materials to guide the constructor of our floating batteries, which may be found in that work, are of so scanty a nature.

We conceive that the theory of projectiles, as applicable to gunnery, previous to the experiments of Robins, was in

a similar state to that of naval architecture at present, in relation to its practical use. In fact, the deficiency of data to teach the science must be admitted in both cases. A theory must be formed from facts. First, the facts must be collected, with all their connexions; they must then be analysed and compared; and, by a clear, penetrating, and capacious mind, inferences must be drawn of general and correct application, or the nearest approximations sought to such general deductions.

By the talents of Robins, the intricacies of the resistance which the air opposes to cannon-balls were elucidated; and it is to similar deductions that we are to look for a penetration through the obscurities which darken the theory of naval architecture. When we turn to Dr. Inman's work, with every respect for the learned gentleman, which was published after he had been Professor to this National Institution only ten years, we look in vain for principles of this kind. From the example of construction given by Dr. Inman, we apprehend, however, that the calculations for ships, formed under his eye, have been nice to a degree, in some immaterial points, while the more weighty elements have been overlooked. Nothing is more necessary in the construction of ships, as in all other things, than duly to appreciate the importance of its different component parts and uses, according to the amount of their effect on the whole: to refine on minutiæ, while the elements of great consequence are neglected, is a mode perfectly erroneous; they may follow after, but ought not to go before. In order to determine these things, we must say that our theories of hydrodynamics and pneumatics are insufficient for the purpose. To the analysis and comparison of ships, as proposed by Mr. Major *, and approved by the Navy Board, we turn with great expectations: some deductions, with respect to increasing the dimensions of ships, have already been made and acted on, from that mode of pursuing the subject, which tend greatly to improve our navy; and by pursuing the subject in that way, we may hope to be placed at that pre-eminence in the art which our nation's wants require, and render so highly desirable. When the plan was first proposed (five years ago), visionary and merely speculative notions were attributed to it: the examination of ships has since been found to be the true source of improvement; and while we see alterations made, we cannot but regret that

* For a more particular account of this plan, see *Annals of Philosophy* for November 1825, January 1826, and June 1826.

they are made from partial views of the subject, and in many cases from incomplete and incorrect data. If the theory of Naval Architecture had been cultivated in England as in other countries, a Digest of the Navy would not have been so requisite, as calculations to a considerable extent are made in foreign maritime nations; but, in this country, we are very hazardous in our procedures without it. "Experience is a dear school," and it is the object of wise men to foresee its dictates, and profit in the future from the experience of the past, before they feel its severe lessons, which often come too late for their beneficial operations.

Whilst, however, we are speaking of experience, we must distinguish between experience in ship-carpentry and experience in the proper formation of ships for their destined purposes. In barbarous countries, fastening pieces of wood together for the mere purpose of forming floating bodies, or the imitation of other ships, with a crude idea of their fitness for sailing purposes, forms the whole of ship-building; but in civilised countries, where large ships of war are built at a vast expense, the deduction of principles, and the application of the exact sciences to the art, becomes an object of the very first importance. It is, therefore, with much satisfaction we look to the institution before spoken of—the School of Naval Architecture.

The superintendence of the practical carpentry of our floating citadels, together with that of their stores, &c., was inevitably attended with great expenses, especially in time of war; but it is to be lamented that, until the year 1808, none was avowedly bestowed for the purpose of insuring the excellence of the sailing qualities of such costly fabrics. On the contrary, every thing seems to have been left to chance; and, with regard to any foreknowledge of their properties, we were left to float in the immense regions of uncertainty: they were therefore as likely to turn out bad as good, excepting when we submitted to the humiliating necessity of making servile copies of foreign ships, which were so dearly earned by the gallantry and skill of our seamen: thus copying those models which they had abandoned for improved designs.

We observe that the conductors of the "Papers on Naval Architecture" still cling to obsolete theories of resistance, and recommend pursuing the mode of conduct of the "Society for the Improvement of Naval Architecture;" which, as we have before noticed, only produced disappointment, though carried into effect with accumulated

talent. For our own parts, without waiting to show the extreme improbability of success by this means, we give our decided preference, in the present state of the science, to devoting our energies to the application of the inductive philosophy to *ships*, in which the *whole* of the causes and effects are embodied. While on the subject, we cannot but remark the want of judgment shown by these gentlemen in underrating the truly valuable experiments of Romme*,—experiments which were made on a large scale,—were repeated, and received the approbation of the Royal Academy of Sciences of Paris.

We perceive that the conductors of the "Essays and Gleanings on Naval Architecture" have avoided pretending to develope the intricate laws of the resistance of fluids; and for this we commend them. We are naturally led here to say a few words on this subject, which must be very limited. Newton was decidedly the best acquainted with this subject, as has been clearly shown by our eminent English authors, Dr. Young and Professor Robison, and admitted by the great foreign mathematicians. By experiment, and the application of mechanical laws, he investigated the subject to a very great extent. He proves, and pronounces positively, that depth from the surface has no additional effect in producing resistance in water. Now, we cannot but express our surprise that, after his clear experiments, an attempt should be made to introduce the contrary notion of Don Juan. It must be remarked, that Don Juan does not himself apply his own theory to ships, nor show its correspondence with experiments that are subsequently detailed in his book—and yet the student of naval architecture is to be perplexed with all the undigested opinions of a verbose and confused writer, who has not discovered one new problem, but has put forward and supported numerous errors. Without wasting time on this subject, we remark, that there is a great difference between a mathematician writing on resistance, and a naval architect; the one, *if* he displays additional powers of mathematical analysis, advances abstract knowledge; the other, unless he succeeds in making the practical application to ships, totally fails.

The "Essays and Gleanings" was designed, by a familiar and interesting mode of treating its subjects, to obtain a general circulation in all the various ship-building yards of this country. It is not commonly known that, even to the

* The "Papers" in the 3rd Number retract this error.

present period, the English ship-builders are not even furnished with proper rules for the mensuration of the bottoms of ships. Their practice, when they wish to measure the solid contents of the submerged part of a ship, is to divide the bottom, by model or draught, into sections of a foot apart; they then divide these sections into square feet, by ruling lines at a foot apart, and *count the number of square feet*, adding *something* for the *gore* pieces, or angular parts at the edge of the sections; afterwards, they add all the square feet together, and allow a *little more* for the corner pieces, for the whole number of cubic feet in the bottom!!! We need not say, that the mathematician is in possession of short and correct rules, as a substitute for this inaccurate, although monstrously laborious method: these rules, by means of this publication, have been made available to all concerned in such computations.

The first number contains introductory remarks, diagrams and explanations of Captain Philips's capstan, in which the principle of the contrivance, understood by few, is lucidly shown;—a theoretical view of the influence of length on a ship, being an analysis of Du Hamel's remarks on the subject;—the values of ships of all rates, as estimated by the French, in the "Archives Navales;"—account of the durability of French ships, from the same work;—notice of the performance of the Enterprise, steam-vessel, in her voyage to the East Indies;—and finally, a very luminous historical view of the origin, rise, and progress of steam-navigation. The variety of subjects thus embraced, it is evident must give a peculiar interest to the publication. In the same manner the contents of the other numbers might be separately cited, of which there are fifteen, to show that the work offers attractions not exclusively to the theorist, nor to the ship-builder, but to all concerned in the naval arts. We especially notice Rotch's lever-fid, Sir Robert Seppings's diagonal system of ship-building, and other late introductions, given, as projected by the inventors, with further explanations.

There is also a peculiar benefit attached to the plan of this work, which is, that scarce documents, naval tracts out of print, are reproduced at a cheap rate. Of these, are to be found the Outlines of Naval Philosophy of Sir William Petty, with an interesting account of his life; and Sir Walter Raleigh's valuable notes on the British navy, &c. Amongst the original articles, is a good account of the origin and advancement of naval ordnance, and new and concise de-

monstrations of the various problems of the stability of floating bodies, with their practical applications to ships. The tract on the stability opens with an interesting historical account of this branch of hydrodynamics, from Archimedes to the present day. For our parts, we are always advocates for this manner of engaging the attention of the reader in the cause of science, which renders even the most abstruse subjects, in some degree, familiar with every body. We thus enlist in its service the prime mover of human intellect —*curiosity;* and having once done that, investigation closely follows. We see this method now employed in almost every scientific treatise; and much of the wonderful increase of knowledge of these periods must be ascribed to it. The subject of the disquisition under consideration is divided into four sections; the first section consists in the enunciation of definitions and elementary truths; the second section contains a very clear, and in some degree original, method of investigating the laws of the equilibrium of floating bodies generally, when deflected from their quiescent positions; the equilibriums of stability, instability, and of insensibility, are clearly defined, and made comprehensible to moderate capacities. The opening of this section strikes us as being of general importance.

"In preparing a draught for a ship, it becomes an object of primary importance to ascertain and regulate, as we proceed in its construction, the properties which it would give to the vessel, and especially whether she would be sufficiently able to restore herself to the upright position, after having been deflected from it by the usual causes. If a draught be not constructed with due attention to this point, a ship may be built which will prove inefficient in boisterous weather, and endanger her own safety, and the lives of those on board. An accurate knowledge of the laws by which the equilibrium of floating bodies is governed, is therefore highly necessary to the constructor; and, indeed, every person intrusted with the command of a ship ought to be acquainted with, at least, the general principles by which the stowage, especially of merchant-ships, should be governed. If we suppose, for instance, that a ship is sailing with the wind abeam, or in other words, propelled by a side wind, the motive power not only gives the vessel head-way, but also inclines her from the upright; if, therefore, this last mentioned effect be not sufficiently resisted, she will be in danger of upsetting in sudden and violent squalls; and when under a press of sail, will always heel so much, that the wind will

lose a considerable portion of its impelling power, from the obliquity with which it must strike the sails; hence, *cæteris paribus*, the velocity of the vessel must also be diminished. Thus, whether safety or good sailing be regarded, we see how important it is to insure a counteraction to the inclining power of the wind."

The third section comprises all the useful problems in Atwood's paper on the subject, printed in the "Philosophical Transactions of the Royal Society," for 1798; but a more general and elegant analysis has been used, by means of which the greater part of Atwood's Paper has been compressed into a few octavo pages. It also gives two concise rules for the mensuration of all irregular bodies. These rules are deduced, not from the method of differences, as is usually done, but by a simple algebraical process, which can only be appreciated by those who are conversant with the higher branches of this science.

The author of this tract then proceeds to give the method of determining the displacement, &c., of a ship from its design, which is effected in a manner intelligible to any person acquainted with arithmetical operations; and there can no longer, in our opinion, be any excuse, even for the most ignorant shipbuilder, for neglecting the determination, at any rate, of that important element of a ship—the displacement.

The fourth and concluding section, professing to treat on the metacentre and metacentric curve, is not yet published, but we hope soon to see it. It is now well known, that Atwood, in controverting the propositions of Bouguer, has printed glaring errors* in the "Philosophical Transactions," while he has properly investigated the stabilities of the isosceles wedge, in its upright and inverted states, which Clairbois has erroneously discussed.

The enterprising, learned, and remarkably acute philospher, M. C. Dupin† (since created peer of France) has not escaped particular notice in the "Essays and Gleanings." His account of the "Progress of the French Marine, since the Peace," which was read in March, 1820, before the Royal Academy of Sciences, forms the subject of an article, which

* In the observations on Atwood's Paper in the "Papers on Naval Architecture," these *errors* are not admitted, but their justification is attempted by the conductors.

† Dupin is the sixth peer created in France, of late times, for eminent attainments in the sciences and literature—such is the encouragement to science in that country.

deserves considerable attention. On the instruction of the French artisans, we extract the following:—"The system of mutual instruction among the working classes prospers in the bosoms of our ports, and schools are founded for the particular instruction of the sons of the inferior officers of the arsenals, in the elements of calculation, of geometry, and of design, as far as necessary for the plans of ships; also the principles of statics, so as to enable them to judge of the action and effect of machinery. Prizes of gold medals and special promotions are to be the rewards of the most deserving students.

"The plan of these schools, which may be cited as models, is due to M. Tupinier, subdirector of the ports and arsenals of France.

"With regard to the creation of establishments of general utility to the progress of the marine, Brest was formerly the only port properly furnished with them. Since the peace, however, libraries are forming in each of the others; and, in almost all, cabinets of natural history and botanical gardens are enriched at every voyage undertaken by our ships, either to foreign coasts, or to those of our colonies."—"An observatory has been given to Toulon; and Rochfort is to be similarly enriched. In both these ports naval museums are formed, in order to preserve types of the most eminent vessels, whose originals either are, or soon will be, destroyed by time. Models of ingenious machines, representations of interesting manœuvres, a methodical collection of raw materials, of tools, and of the product of all the arts exercised in a dock-yard,—such are the riches collected in these interesting repositories."

We are of opinion, that the establishment of institutions of learning, similar to those of France, at each of our three great naval arsenals, Portsmouth, Plymouth, and Chatham, would be highly beneficial to the country. Advocates of a fallacious economy would deplore the expense of such national undertakings; and the malignant opposers of advancement in knowledge would also object to it; but it may be asserted, that past experience, and a present view of naval affairs, loudly call on the country for some struggles for our attainment of superiority in naval science. Who could remark a young naval power, like America, possessing superior vessels to our own, in the last war, without perceiving the necessity of this step?—it reverberated through the houses of parliament, with just complaint.

At the present time it is well known that ships of the United

States possess a far superior character to our own. How many expensive alterations have been rendered expedient in our ships, that a precedency in the theory of naval architecture would have rendered unnecessary! Of what purpose is it to copy models in the present day, which were taken from foreign nations thirty years ago, and are of a still earlier date in point of design? It is thus that we have been always a step behind them: improved models, we know, replace, in the French navy, those of ancient construction.

It is true that we have some names among us that are cited as those of good ship-builders; but it must be observed, that as tapers shine in the dark, which are dimmed by the approach of day, so it is impossible to determine the relative merits of our naval artists till open competition shall duly distinguish them. That time is now approaching: nay, from the enlightened and judicious measures of our naval administration, is now ushered in. Our public experimental squadrons, in which direct contrast and impartial trial alone decide on merit, are as fair and beneficial modes of advancing our attainments in the arts as probably could have been devised. Again, we have a corps of naval engineers, who are admitted into the service, solely on their general capacities and mathematical abilities. At the School of Naval Architecture they pass public examinations every year, the accounts of which are communicated to the Lords Commissioners of the Admiralty; and it is in the order in which they stand at their final examination, under the Professor, that they have hitherto been promoted. Although nothing can be conceived more impartial than this plan at the present period, yet it must be allowed that it does not give the desired excitement to perseverance in science, subsequent to their departure from the institution; and we have no doubt, for this reason, that some distinction will be made for remedying this deficiency: already the theorems and principles for ship-building have been explored to an extent far surpassing what was known seven years since at the School of Naval Architecture.

It must be remarked that superior genius, where it does not meet with protection and reward, is always productive of much real injury to the possessor: as an instance of this, we cite the contempt and obscurity in which Mungo Murray, our only native writer on the theory of ship-building, lived and died, which is related by Mr. J. Knowles, the present Secretary to the Committee of Surveyors of

the Navy, in the preface of his work on the "Preservation of the Navy."—"While the Dutch possessed and encouraged Witsen,—the French, Bouguer, Du Hamel, Clairbois, Borda, and Romme,—the Spaniards, Juan,—the Germans, Eüler,—and the Swedes, the celebrated Chapman; the English neglected the only work which they possess on this subject, that can lay any claim to science*, and suffered its author, whom tradition represents to have been a man of the most amiable manners and correct conduct, to live and die a working shipwright in Deptford Yard." In the following page, in adverting to the students of the School of Naval Architecture, he says, "it is to be hoped that their ardour may not be damped, but that the encouragement will be extended to them which has hitherto, in this country, been withheld from those who have endeavoured to promote the difficult but useful science of constructing ships."

On the paper inserted by Sir Robert Seppings, in the *Phil. Trans.* for 1814, on his diagonal system of ship-building, which is here reprinted for convenient circulation, it behoves us to make a few remarks. The adoption of this plan is a great triumph over inveterate prejudices, and we are inclined to bestow praise on the introduction of this new mode of carpentry; but we do not conceive we should abandon investigation respecting it. The late writers on it, in addition to Sir Robert, are Baron Dupin, Dr. Young, and Mr. Knowles. Of these Dupin, being a naval architect, has by far surpassed the others in its discussion. Dr. Young's more limited acquaintance with ships has necessarily confined his illustrations, though his elegant analysis has much enriched the subject. Mr. Knowles's descriptive view is unassisted by mathematical investigation.

Dupin remarks, in his paper, in the *Phil. Trans.* of 1816, that the principle has often been recommended in theory, but found deficient in practice. He says, introducing the subject, "The old French builders saw so distinctly the truth of the principle now produced by Mr. Seppings, that they even put it in practice for the same purpose to strengthen their ships, and to prevent arching." Again, "Chaucôt, a French ship-builder, obtained the prize, in 1755 (of the Royal Academy), and in a Treatise very little known, reproduced the idea (of Bouguer and Du Hamel) on this point." In subsequent remarks he observes, that Groignard, in 1759, proposed, and that Clairon des Lau-

* *A Treatise on Ship-building and Navigation*, by Mungo Murray, 1754.

riers put in practice, in 1772, similar principles. The celebrated Chapman also applied it.

We conceive that the subject cannot be decided *à priori;* but must be referred to general principles in ships. Hence, Dupin remarks, its superiority must be determined by showing that the efficient ship of war, on Sir Robert's adaptation of this principle, is constructed with less timber and fastening than on the old plan; that it has the weight placed lower; that it is of less expense, or has improved qualities. Now, we cannot avoid remarking, that this has never been shown, but only asserted; and that there is a great deficiency of calculated data on these points. Dupin himself only hazards an opinion on the subject; but, as a mathematician, he ought not to have been satisfied with anything less than an accurate statement. He passes over the new plan of attaching the beams to the side, as of little consequence, without showing it to be attended with less weight than the old methods; whereas they must be estimated in the comparative weights of the ships. It is evident, from mere inspection, that the new mode is the heaviest, and that it contributes, in too material a degree, to raise the centre of gravity of the ship, and diminish its stability. To determine the efficacy of the diagonal riders to prevent arching, Dupin ought to have shown that they are equal, for this purpose, to the internal coat of planking, so boldly dispensed with by Sir Robert, independent of the massy interior ranges of timber, between which the beam-ends are confined; for it is evident, that these strong longitudinal binders would of themselves effect the same object in a very material degree. The shelf-pieces, which were used by former English surveyors, are not of English introduction, nor of French solely; as, by reference to ships constructed in the Archipelago and the Levant, they will be found to have been used for ages.

We think it but justice to observe, that when some new mode of fastening the beam-ends was required from the scarcity of warden-knees, that Mr. Roberts proposed a plan which was found to succeed so well as to obtain for him a parliamentary grant of 800*l.* Whether Mr. Roberts derived it from Du Hamel we are unable to decide—there is a similarity in the plans; but the objection urged against the plan by Mr. Knowles, of the beams of free growth being split by the bolts in the beam-arm, is certainly futile; as it was easily remedied by placing them out of the same line of fibre.

A great deal of irrelevant demonstration has been injuriously applied to prove the excellency of the diagonal riders. There are no sources of error and confusion so prolific as inappropriate analogies. To liken thirty bending riders, in the curved bottom of a ship, to the straight cross-bar of a common field-gate, is an assimilation so unapparent, that we are surprised how it could ever be adduced *. If the side of a ship were perpendicular, and one cross-timber reached from the hold in midships to the extremity forward, and another aft, the comparison would be more admissible. But, in that case, there would still be great dissimilarities: the side of a ship is planked from the top to the bottom, and this coating is so close, as to prevent the admission of water; while the bars of a gate are only intended to prevent cattle getting through. A ship is six or seven times in length of hold what it is in depth; whereas a gate is only about twice the length of its height. In the case of a ship dropping at its extremities, the upper-works must lengthen; whereas, in a gate, this deformation takes place by a revolution on the fastenings, by which the bars approach each other, and which the planks of a ship are prevented from doing by their immediate contiguity.—If this train of reasoning can be allowed, it will only apply to the mode of placing the diagonal riders, adopted by surveyors, previously to Sir Robert, as exemplified in the top-sides of the Caledonia,—a ship which has received an universally good character. But the question can only be determined, as we have observed, by general results.

Some persons have supposed, that, in the diagonal system, the strains are transmitted endwise on the fibres, in which direction wood is the strongest; but it is evident, that if the enormous angular momentum, producing the working of a ship, depended for resistance on the bolts of a few cross-riders, the strain would not be so well supported as by the multiplicity of bolts in the thick stuff and ceiling covering the concavity of the hold; for this reason, we find Dupin, in preference, recommending interior diagonal planking between wind and water.

The resistance to arching must depend on the distance at which the opposing force is applied from the neutral

* A ship has been likened in this discussion to various other articles; even *Indian rubber* has been brought forward as an analogy, the application of which we do not exactly perceive. By comparing it with the moon, or any thing else, we may obtain any imaginable results we may desire.

axis. Thus, the upper planks afford more strength than those at the water's edge, which cannot come into action till those above or below are considerably stretched or compressed. For this reason Dr. Young observes—"The least obvious advantage attributable to the obliquity introduced by Mr. Seppings, appears to be in his mode of laying the planks of the decks; parts which seem to be principally required to co-operate with the sides of the ship as ties in a longitudinal direction."

The fillings between the timbers certainly cannot afford any strength to the ship; their resistance to compression is not equal to that of a coat of planking, which acts in the direction of its fibres, with numerous fastenings, both to prevent compression and extension—the latter of which the fillings can have no sort of pretension to. As the ship is not filled up to the floatation, water is not excluded by them in all cases from entering the ship. Their introduction only became necessary when the ceiling was dispensed with; and thus forming a part of the new system, they must stand or fall with it. It has been adduced in favour of filling in a ship, that the weight being below the centre of gravity, it must act as ballast: to this fallacious recommendation it need only be observed, that one ton of ballast, placed at the bottom of the hold, would have the effect of two or three tons placed in the situation of the fillings.

It must also be recollected, that an objection has arisen to the interior planking on account of its weight, while the same has not been alleged of the fillings; but Dupin, who notices this, sometimes writes with mere political views, without a scrupulous regard to correctness; for the truth of which assertion we need only adduce his account of the British treatment of French prisoners, and his omission of taking the weight of the new fastenings of the beams into consideration, so properly descanted on by Dr. Young, which was mentioned before.

We cannot conceive with what propriety, persons can say that a coat of planking contributes little to the strength of a ship, when it is merely the four-inch planks of the bottom of a ship that bind together and protect three or four thousand tons of heterogeneous matter from all the ravages of the winds and waves. Perhaps, there is no parallel to this prodigious combination of strength, with disproportionably small substances, in the whole range of the arts: the arch and the hoop, where the separating forces are equally distributed and counteracted by the continuity

of strength, appear to present some comparative idea of the accumulation of its strength; but it is in the construction of the egg, in which the wisdom of the Divine Author is exhibited, that we must probably look for the nearest resemblance to it. The decks of a ship contribute above water, in the straight part of the planking, to maintain the form, but its sole security under the water depends on that peculiar species of support mentioned above.

It is proper to advert to one circumstance, hitherto not noticed, in the discussion of this important subject, and which appears to destroy the whole chain of reasoning adduced for showing the strength of the diagonal system:—we allude to the shrinking of the principal riders, against which the secondary or truss timbers abut. This necessarily leaves a vacancy at the extremities of the trusses, so that they are totally ineffectual for their designed purposes, till plates of iron, or other hard substances, are forced into these interstices—a very common, we are told, and inconvenient practice. Abutments in ship-building are a very bad species of support, as they receive the shocks to which a ship is liable too suddenly; but the diagonal system appears to be destitute even of the abutment, by the shrinking of the principals.

Whether the expense of constructing the navy on this new system is greater or less than on that of the old, we are unable to determine.—We are glad to see a mixture of the diagonal riders and the ceiling now adopted in frigates, instead of all wooden riders.

As a convincing proof of the excellency of the diagonal mode of placing part of the timbers of a ship, it has been adduced that the French have again adopted it for trial, in a few instances; but here it must be remarked, that with them time has not elapsed for the demonstration of its value; nor have wars, and other circumstances, occurred, to decide the question. Our English merchant ship-builders, in several cases of its adoption, have proved its inefficiency: the steam-vessels, now built with it, sag, or drop down in the middle, from six to twelve inches; and we fear, on a full view of the subject, its boasted advantages to our navy, must fall to the ground.

A more complete examination of the cross-carpentry of Sir Robert Seppings, than the sketches hitherto before the nation, is much required; and as it involves the construction of an immense navy, which costs twenty millions sterling to replace, we hope it will soon appear.

It is not so much in a longitudinal direction as in athwartship one, that ships are weak. It would appear that this was felt to be the case by Sir Robert Dudley, at the latter end of the sixteenth century, when he proposed his construction of ships from the galleon downwards, and endeavoured to remedy the evil by struts, extending from the keelson to the quarters of the beams of the different decks. Mr. Snodgrass also, as shown in Steel's large work, brought forward almost a similar plan, with this difference, that the upper ends of the struts were fastened to the side of the ship instead of the quarters of the beams. The latter method failed when put in practice, which, we may infer from Charnock, was not the case in Sir Robert Dudley's mode. —*Vide* vol. ii. p. 177, of his *History of Marine Architecture.*

If, however, we do not give our entire assent to all that has been advanced respecting the diagonal system of Sir Robert, we must still yield him the praise due to perseverance in what he believes to be the true principles of construction, and more particularly in that exerted in the behalf of the round sterns of his introduction. We feel persuaded that, whatever modifications it may be found hereafter necessary to make in them, the curvilinear shape will never be abandoned. We should prefer to see them, however, as the French have adopted it, without any external projection, to load and hamper the ship abaft.

It is to be regretted that the accommodations of the officers should be infringed on; but surely, in a ship of war, every thing is of minor importance to her efficiency as such. No doubt, deeply-rooted prejudice in favour of old customs renders it obnoxious to many; and when once that is overcome, opinion will be as violently in favour of the round stern, as it was previously inimical to it.

The remarks on dry rot seem to be written in true philosophical spirit,—a determination to investigate the subject with strict impartiality. We perfectly agree with the opinion of the Noble Lord at the head of the Admiralty, expressed in Parliament—"that all nostrums are ineffectual towards the preservation of timber; and that the durability of it depends on its quality, and on its being well seasoned." For this reason we agree to what experience has taught all ages respecting the time of felling oak-timber; namely, that period when the vegetating juices, after spring and summer, are converted into ligneous fibre, and not when they are in a state of action ascending the ca-

pillary tubes. The high price of oak-bark alone operated against this dictate of experience in the time of James II.; and subsequently has brought into use, even in the Royal Navy, timber felled in the spring. All authors, except Mr. Knowles, agree to this. The reader will find ample satisfaction in the perusal of this article.

We cannot afford space for the mention of other papers, on Steam Navigation, the Centre of Gravity, Blake's Midship Section for Fir Ships, and several practical improvements.

We think His Majesty's navy may derive the greatest advantages from scientific discussion; and whether ignorance, envy, or prejudice, actuate the opposers of it, we assert that they are no true friends to their country. Every empirical introduction, without it, may pass for the extension of science and discovery. But we have too much confidence in our enlightened naval administration, to fear that the admirable beginning that has been made in fostering the true principles of naval architecture will ever be discontinued; but, on the contrary, we believe this country—that of Newton and Locke,—is now destined to surpass every other, as much in the theory of the art, as in the solid construction of her navy.

Remarks on the Principle of the DIVISION *of* LABOUR *as applied to* LITERATURE *and to* SCIENCE. *In a Letter to the Editors of the* BRITISH CRITIC. By a Correspondent.

Gentlemen,

I HAVE read with great interest, and generally with decided approbation, your Review of the works of Mr. Champollion; but there are some particular passages in that Article which have suggested to me a few observations which seem to me to be of sufficient importance to be made public.

My first remark shall be on the passage (p. 152) in which you say, that Mr. Champollion, treading in the steps of Dr. Young, "and enjoying the *very great advantage* of having nothing to distract his attention, advanced still further;" a sentiment which is so natural and obvious, that you would scarcely expect an objection to be raised to it, even by Dr. Young himself, especially when it is compared with an expression respecting

this very study, which he has employed in his letter to the Archduke John, dated in August, 1816 : "They must however remember, when they undertake such a task, that it is not by the gigantic exertions of fancied talents, but by the stubborn perseverance of indefatigable industry, that we can ever hope to obtain, for ourselves and our successors, an admission into the hidden treasuries of nature and art." (Museum Criticum, vii.)

So far therefore as Mr. Champollion possessed more perseverance, or was able to concentrate on his researches a greater portion of industry than Dr. Young, it would of necessity be admitted by this gentleman, that his contemporary had an advantage over him. However this may be, it appears to me that the remark which you have made on the subject implies an opinion which, though blindly adopted by many persons, and very generally applied on various occasions in life and in literature, is not only erroneous in theory, but is fraught also with extensive practical mischief.

The great principle of the Division of Labour, which has been so successfully introduced into commerce and into manufactures, and into mechanical operations in general, and of which the importance has been so favourite a subject of exaggeration with the political economists of modern times, has a tendency to raise the operations of machinery into a rivalship with those of intellect and science ; but I am mistaken if the same principle, when applied too strictly to the human mind, has not also a tendency to depress its faculties into the similitude of mere inanimate machinery; and it seems to me to lower the energy of the understanding, as much as it increases the flexibility of the fingers.

When a man devotes his whole life to a single pursuit, he is quite secure of doing *more* in it than by a temporary application ; but it is equally certain that he has less chance of doing any thing *great*, than by taking a wider and more distant view of collateral objects, and watching his opportunity to start the game, which must rise of itself, before he can possibly hunt it down.

A determination to follow a single object is a most abundant source of the self deception so commonly seen in men employed in the professions. There is a disposition in regular

professional men to call a man a *quack*, who confines his practice, or even his investigations, to a single object; and a sentiment, so apparently the offspring of an illiberal jealousy, is in fact, not uncommonly, founded on the actual experience of the unavoidable effect of such a division of labour, which is, first, to cause the individual to deceive himself, and then to render him an involuntary agent in deceiving others. If, on the other hand, the profession is such as to require any proceeding of a manual or mechanical nature, there is no doubt that this kind of operative excellence must be most essentially advanced by a considerable limitation of the pursuit, though not by a total and exclusive devotion to one object only.

Now if learning were mere book making, and were to be acquired by an immeasurable collection of words and authorities, that man would indeed be the most learned who looked over the most books, and copied the most words into the margin of his dictionary, or into his common place book; and then the devotion of a life to a single language, or a single investigation, would far outweigh, in its cultivation, the possession of any superior judgment or discrimination.

But where evidence is to be compared and appreciated, and where conflicting probabilities are to be balanced against each other, it does appear to me, that a familiarity with some other pursuits, besides those of a mere linguist or historian, becomes of value in making the labourer worthy of his hire; for without them he may labour a great deal, and may yet do worse than nothing.

I am far from wishing to insinuate that all of Mr. Champollion's labours are of this kind, or even to assert that any of them are so. But I cannot help thinking it *morally impossible* that he should not have overrated his own successes; and I must confess that I have not *yet* been able to convince myself that he has made out any thing whatever, besides the GREEK and ROMAN PROPER NAMES, and one or two detached hieroglyphical characters; and I doubt whether I shall be able to satisfy myself, for ten or twenty years to come, whether he is right or wrong in any thing further.

The evidence of what was done in this country is fully before the public, not indeed in a very popular, but in a suffi-

ciently intelligible form: that is, in the INDEX to the volume of Hieroglyphics, "collected by the Egyptian Society," and now continued by the Royal Society of Literature. The brackets, which join the words of this index, point out at once the degree of agreement between the characters supposed to be translated; and by a reference to the plates there indicated, the reader may judge for himself to what extent he may place confidence in each interpretation. Some characters occur, for instance, about a hundred times, some fifty, some ten; some two only: yet all of these may be fully authenticated by the recurrence, if the identity of the forms is sufficiently apparent; and sometimes the context may be such as to leave no doubt whatever, where the character occurs but once. When Mr. Champollion shall have leisure to do that, on the part of his system, which Dr. Young's "distracted attention" has still left him time to perform for his interpretations, we shall be better able to judge with confidence of the real progress that the investigation has made in his hands.

In the mean time I shall proceed to consider, in the second place, the character and internal evidence of the SYSTEM, concerning which so much has been said, (Brit. Crit. p. 142) and which, so far as it is a system, seems to me to contain within itself its own contradiction, for a system of PHONETIC HIEROGLYPHICS never could have been generally employed but by a nation of children. Take, for example, the word APPLE, expressed by an apple, two pears, a lion, and an egg, all denoting an APPLE only! Surely there must have been some common sense among the Egyptians, though they seem to have wanted no grammarians, and are reported to have had neither orators nor poets! And would common sense have permitted them to add four superfluous characters, out of five, in order to be mere echos of the sounds, denoting what is already denoted by the first of the five?

The truth is, that there never was a language that was written *altogether* phonetically. A very numerous class of our own characters are not phonetic: our Roman and Arabic numerals, our stops, our parentheses, our marks of quotation, and many other extraalphabetic signs; and even our literal characters are sometimes not simply phonetic: the sounds of

son and *sun* are precisely alike, so that we are every now and then obliged, in speaking, to translate them into another language, if we wish to distinguish them; but the written words have hieroglyphical significations totally distinct from each other.

It is generally admitted that the Chinese characters were originally, and still are, the signs of things only, and not of the sounds of the words, being even read very differently in different provinces, although in some particular cases, especially those of foreign names, they are of necessity used phonetically, if not altogether alphabetically. Mr. Champollion has very ingeniously shown that the Egyptian Hieroglyphics were *sometimes* used to denote the sound of the first letter only of the name of the thing depicted: but he has not shown that they were *generally* so used, when they were employed phonetically; and it is *certain* that they were *not always* so read: the symbol representing AMUN, or Jupiter, for example, was seldom if ever used for A, but very commonly for MN, as Dr. Young has observed. So much for the *universal* application, which you give Mr. Champollion credit for having discovered (p. 146).

With respect to the passage of Clement, so often quoted, his words, in my opinion, *could* never have conveyed to a *Greek* the sense attributed to them by Mr. Champollion; nor do I believe they were ever so understood by himself or by any other person, until he had analysed the alphabetical names of the Roman emperors, and obtained from his investigation the interpretation which he assigns to the passage. What Clement did mean I do not pretend to explain; my conjecture is, that like many other people, he talked of what he understood but very imperfectly, and was therefore likely to express very obscurely. If, however, he *had* heard accidentally of the manner of expressing foreign names which had become usual in his days, it would add little or nothing to the probability that the same system could have been adopted two or three thousand years before; and the *disagreement* between Manetho and the tablet of Abydus is sufficiently *obvious*, to my apprehension, to destroy any inference in the affirmative from his authority. But, as Dr. Young has somewhere observed, (Journ. R. I.

Jan. 1823,) " It is better to make a few false steps than to stand quite still."

There are some detached passages in your review, on which I shall make a few remarks of a more miscellaneous nature.

P. 143. " There is no reason to ' dispute' Mr. C.'s having, in many cases, *anticipated* Dr. Y.'s discoveries." Now Mr. C. does not profess to have *anticipated* anything that was done in England: either he denies the accuracy of some of the conclusions, or he professes to have been unacquainted with them; but he does not suppose Dr. Y. to have made out any thing which he had made out *before:* in that case the question might easily be settled by priority of publication.

In the same page, you seem to take for granted that Quatremere has been handled unfairly by Champollion. But are there three persons in England who are capable to decide that question?

P. 145. " The hieroglyphics might be arranged from right to left, or from left to right; but the *latter* is the most prevalent order." It is not easy to say which order is prevalent in the sculptured inscriptions; but it may be questioned if the Egyptians *ever* wrote from left to right with a pen. And this fact *gives* some authority to the accuracy of Herodotus.

P. 150. When you say that " the kind of writing, which expressed objects, by means of the letters of the alphabet, is manifestly the enchorial of the Rosetta stone," you seem to have forgotten your admission (in p. 144), that " Dr. Young ascertained that the Egyptians had no [purely] alphabetical characters whatever."

P. 154. " The connexion between the hieroglyphic and alphabetic characters is pointed out, if we mistake not, in the Supplement of the Encyclopædia, published in 1819." It appears to have been *first* pointed out in Dr. Young's letter to the Archduke John, dated August 1816, where he observes, that " a loose imitation of the hieroglyphical characters may even be traced, by means of the intermediate steps, in the enchorial name of Ptolemy."—P. 3. Mus. Crit. vii.

P. 156. You say that Petammon must probably be the Egyptian word corresponding with Ammonius in Greek; and that the supposition becomes a certainty from the Greek in-

scription on a mummy. It is, however, remarkable, that the words are *not* used as synonyms, but as distinct appellations, which may be inferred from the ALSO of the inscription; and Ammonius is written distinctly in Mr. Grey's enchorial manuscript, where we might have expected to find Petammon, if Ammonius had been merely a translation.

P. 162. "M. Champollion does not seem to have heard of Mr. Bankes's copy of the Table of Abydos." On the contrary, Mr. C. distinctly states, that he had first heard of this tablet from Dr. Young. The accuracy of Mr. Bankes's copy is confirmed by that of Mr. Wilkinson, which is printed together with it in the last fasciculus of the "Hieroglyphics:" a work which contains a multitude of proofs of Mr. Wilkinson's great industry and accuracy in preserving inscriptions so unaccountably neglected in the great work of the French antiquarians; and which will contain a still greater multitude of his communications as it proceeds.

Mr. Champollion's words are these:—"Tableau précieux, dont une copie est depuis plusieurs années dans les portefeuilles de M. W. Bankes, en Angleterre. Je dus le premier avis de son existence à l'amitié de M. le Docteur Young." Lettre première au Duc de Blacas, p. 15. This passage has also been overlooked by the learned author of a late article in the Edinburgh Review.

I am, Gentlemen, your very obedient servant,

* * * *

Remarks upon the Orchideous Plants of Chile. By John Lindley, Esq., F.L.S., *&c. &c.*

MUCH as has been done in investigating the vegetable productions of South America, it must be confessed that we still are far from possessing any very definite notions of the botany of that vast continent. But if this be true of the South American Flora in general, it is much more so with respect to the more southern provinces, and especially with regard to Chile. The number of plants recorded in scientific works, as natives of that region, are so few compared with what from analogy it may be

presumed to contain, that its botany may be considered almost entirely unknown. It is true, indeed, that a very considerable addition to the published lists of Chilian plants might be made from an examination of certain private herbaria; but as far as the scientific world in general is concerned, the statement above made is strictly accurate.

From the copious materials relating to the vegetation of Chile which exist in this country, much information of the most important nature is to be derived; but from no source so extensively, perhaps, as from the collections formed for the Horticultural Society by Mr. James M^c^Rae. From these I have selected the highly curious tribe of Orchideous plants as the subject of the following remarks.

The distribution of Orchideæ with respect to the surface of the globe, is one which has hitherto scarcely occupied the attention of geographical botanists; nor can our notions respecting it, for some time to come, be expected to acquire any considerable degree of accuracy. So much difficulty exists in preserving dried specimens of this tribe of plants, and in the determination of them when preserved, that, with the exception of Europe and North America, we can form no conception of the proportion that they bear to the Flora of the world, or of the relation which the forms of one Flora bear to those of another.

A few striking facts appear, however, to be ascertained, which may serve as the basis of future observations; and which are well worthy of consideration, as they seem to indicate between the minuter parts of the structure of Orchideæ, and their general economy, a close connexion which is not at present susceptible of explanation.

In all extra-tropical countries, where the species grow on the earth among grassy herbage, and are supported by fleshy or fasciculated roots, the greatest number belongs to genera with decompound pollen-masses, or, more correctly speaking, with their pollen in a very incomplete state of cohesion;—those with solid pollen masses and glandular appendages either not existing at all, as in Europe and the North of Asia, or constituting not more than a thirteenth part of the whole, as in North America, and, perhaps, the Cape of Good Hope; and those with

solid pollen, without appendages, not exceeding that proportion, or being altogether unknown, as in the southern point of Africa. Within the Tropics this order seems to be reversed: there the species are, for the most part, found upon trees, or growing on decayed wood; their roots are no longer fasciculated and fleshy, and fitted for deriving sustenance from the earth, but filiform and dry, and adapted to clinging to foreign bodies for support and nutriment; in these the pollen is solid, and generally fitted with a glandular apparatus. But whether in intra or extra-tropical countries, it seems to be a general law that species are everywhere to be found with their pollen and columna in five at least of the seven different modifications of which those organs are susceptible. In this I do not include Cypripedium, which is a genus altogether of a peculiar character, and only known in the Northern hemisphere.

To the law just stated I am aware of only two exceptions—both very remarkable; the one being the Cape of Good Hope, and the other Chile. In Southern Africa, the Orchideæ consist almost entirely of Ophrydeæ, but of a structure quite peculiar to that promontory; there is only one Neottiea and a very few Vandeæ. The genera are all strictly what M. De Candolle calls endemical; they are numerous and extremely dissimilar the one from the other, while no immediate affinity is to be traced between them and the genera of other districts, except the Isles of France and Madagascar, and the more equinoctial districts of the African continent. Upon the whole, whatever peculiarity may be found in the colony of the Cape of Good Hope as to the genera which inhabit it, there is no great deviation from the general proportion which genera and species bear to the surface of the district they occupy.

But with Chile the case is widely different. In this country, which, in the eyes of botanical geographers, forms a vegetative region of itself,—which extends through twenty degrees of southern latitude, and which has a surface varied with all the irregularity of mountains covered with eternal snow, rich vallies, and extensive plains, no more than three species of Orchideæ are at this moment recorded to exist, and these are known only by the figures and imperfect descriptions of Feuillée.

To this number I have now to add nine new species, together with some observations upon the three previously recorded. Upon referring to the end of these remarks, it will be seen, that after taking into account both the published and unpublished species of Chilian Orchideæ, that portion of the Flora comprehends only four genera, three belonging to Arethuseæ, the other to Neottieæ; and there is strong reason to believe that these, if not the only orchideous genera to be discovered in Chile, do constitute almost the entire feature of that part of vegetation which they represent; for among the extensive collection of drawings made by Mr. Miers, during a long and active residence in the country, I observed nothing to contradict such an opinion. Of these Chloræa, Bipinnula and Asarca are absolutely confined to the province, and to that of Buenos Ayres; while Spiranthes is one of the most widely diffused of all known genera, species of it having been found in the temperate parts of Europe and Siberia, in Northern India and New Holland, in the islands of the Indian Archipelago and off the coast of Southern Africa, in North America and the West Indies, in Brazil, and lastly, in Chile: in fine, there are not more than five of the twenty botanical regions into which the surface of the globe is divided by writers upon botanical geography, in which Spiranthes is not known to exist; and analogy leads us to believe that in three of these five it will be found by future inquirers.

It is highly curious that the botanical affinities of Chloræa, Bipinnula, and Asarca, the *endemical genera* of the province, are not so immediately with other South American genera as with those of New Holland. While they totally disagree with Stenorhynchus, Cranichis, Ponthieva, and Prescotia, all exclusively American genera, they represent, on American ground, the numerous tribes of Arethuseæ peculiar to New Holland, and especially Lyperanthus and Caladenia. Chloræa galeata has very much the appearance of the New Holland Pterostylis rufa in a gigantic state; and even Prasophyllum fimbriatum of the same country has a closely analogous structure with that of Bipinnula plumosa.

I now proceed to state the technical characters and botani-

cal history of the plants which are the subjects of these observations:—

CHLORÆA.

Collectanea Botanica in App. No. 12. *et Orch. Scel. sine charactere.*

Char. Gen. Pollinia duo pulverea bipartita, caudiculâ glandulâque nullis. Anthera terminalis opercularis bilocularis, loculis completis septulo incompleto bipartitis. Stigma transversum ovatum in apice columnæ. Columna semiteres membranaceo-marginata. Labellum anticum cucullatum basi cordatum, cum columnâ levissimè connatum, cristatum, lobo medio sæpissimè carnoso. Sepala subæqualia ringentia membranacea venosa: supremo galeato; interioribus sub galea conniventibus; exterioribus labello suppositis apice, anamorphose repressâ, carnosis dilatatis. *Herbæ terrestres* (Americæ australis temperatæ) *radicibus fasciculatis. Folia plana, radicalia, venosa. Scapi multiflori, terminales, squamosi. Flores subherbacei (undè nomen.)*

This is a genus, the characters of which have not been before defined. It is only known to botanists as a name that exists in the list of Orchideous genera printed at the end of my Collectanea Botanica, with a reference to the "Epipactis amplo flore luteo vulgo Gavilu" of Feuillée. At the time that work was published, I had only examined one species, from the inspection of which I referred the genus to Neottieæ. An analysis of the species now about to be described has shown me that it ought rather to be placed with Arethuseæ; if, indeed, these two sections are really sufficiently distinct. The affinity of Chloræa to Lyperanthus and Caladenia has been already noticed. From these, and all other known genera of the same Natural Order, it is obviously distinguished by the incrassated, often dilated apices of the two lower sepals, and of the labellum.

1. *C. disoides;* sepalis inferioribus porrectis apice emarginatis attenuatis, labello unguiculato cristato: lobis lateralibus cucullatis membranaceis nudis, intermedio carnoso glanduloso.

Spontè crescit circà *Valparaiso:* Februario florentem legit M^c^Rae, (v. s. sp.)

Leaves erect, acute, sheathing, as long as the somewhat leafy scape, which rises to the height of six or eight inches. *Squamæ* of the scape membranous, inflated, acute. *Flower* ringent. *Sepals* membranous, veiny, the three upper conniving, erect, lanceolate, somewhat obtuse; the two lower spreading with a tapering, fleshy, channelled, obtuse end. *Labellum* stalked, cordate, cucullate, three-lobed, continuous with the base of the columna; the two lateral lobes shorter than the middle one, membranous, veiny, with the anterior margin crenated and callous; the middle lobe ovate, abruptly acuminate, obtuse, fleshy, crested in the middle with numerous fleshy appendages;

the disk with seven rows of interrupted excrescences, the lateral rows being shorter than the rest. *Columna* erect, long, linear, with a membranous margin.

? 2. † *C. Gavilu.*

Epipactis amplo flore luteo vulgo Gavilu. *Feuillée. Peruv.* 2. *p.* 29. *t.* 18.
Cymbidium luteum. *Willd. sp. pl.* 4. 106.
Spontè crescit in *Chile*, locis humidis. *Feuill.*

This is extremely like the last, from which it is certainly possible that it may not be different; I, however, have seen no specimens which fully agree with Feuillée's figure; but while I think it necessary to separate this from C. disoides, till something further shall have been ascertained, I am also bound to add, that the grounds upon which I form my belief are too vague to deserve being particularized.

3. *C. longibracteata;* foliis oblongis obtusis rosulatis, bracteis acuminatis floribus longioribus, sepalis inferioribus ascendentibus apice acuminatis linearibus, labello sessili basi bicalloso: lobis lateralibus planis cristaque depauperatis; medio sinuato linguiformi nudo apice carnoso, columnâ nanâ.

Spontè crescentem juxta *Conception*, Octobre floridam legit M^cRae (v. s. sp.)

A plant with the habit of Stenorhynchus speciosus. *Stem* nine inches high, with long sheathing squamæ. *Bracteæ* with very tapering points, much longer than the flowers. *Sepals* all ascending: the upper membranous, veiny, oblong; the lateral inner ones shorter, but of the same form and texture; the lower ovate-acuminate, erect, membranous at the base, with fleshy, linear, obtuse flat ends. *Labellum* erect, parallel with the columna, sessile, cordate at base, and connate with the columna, slightly pubescent on each side; at its base there are two calli; the lateral lobes are rounded, narrow, slightly veiny, with a single tooth in front; the middle lobe is long, sinuated, fleshy at the end, and veiny along the axis; crest consisting of a few broken excrescences situated towards the base. *Columna* much shorter than the labellum, half round, slightly pubescent, with an erect lobe on each side at the apex.

4. *C. galeata;* bracteis membranaceis ventricosis, sepalo supremo venoso falcato: lat. int. intus glandulosis; inferioribus divaricatis apice cochleatis carnosis, labello ascendente unguiculato; lobis lateralibus cucullatis intus cristatis; intermedio rotundato carnoso nudo, columnâ basi callosâ.

Circa *Cumbre*, Andium claustrum, Novembre floridam invenit M^cRae, (v. s. sp.)

Leaves numerous, obtuse, flat. *Stem* about one foot and a half high, sparingly clothed with withered squamæ. *Spike* lax, six or nine inches

long, with distant flowers, enwrapped in inflated membranous bracteæ. *Flowers* small for the genus, galeate. *Upper sepal* falcate, inflexed, membranous, three-veined; the veins being strongly marked and connected by distinct transverse venulæ; *inner lateral sepals* membranous, obovate-rhomboid, unequal-sided, veiny, at the base obliquely attached to the columna, with the veins in a high degree varicose; *lower sepals* at first erect, then divaricating, oblong, with broad, dilated, fleshy apices. *Labellum* ascending, with a long incurved unguis, which has a double tubercle at the base; the lateral lobes cucullate, each with two crests on the inside, of which the innermost consists of a single lamella, and the exterior of several glandular excrescences; the middle lobe rounded, fleshy, naked; crest nearly obsolete, represented by three obscure raised lines near the base. *Columna* erect, clavate, with a membranous margin, having a large callus at the base.

The resemblance of this species to a gigantic Pterostylis rufa has been already noticed.

5. *C. lamellata;* foliis erectis acutis abbreviatis, bracteis flore longioribus, sepalo superiore lanceolato venoso; inferioribus porrectis apice dilatatis spatulatis carnosis, labello oblongo unguiculato membranaceo crenato, crista lamellata.

Circa *Conception*, Octobre florentem invenit M^c^Rae, (v. s. sp.)

About a foot high. *Leaves* very short, erect, acute. *Scape* leafy at base, clothed with numerous somewhat inflated squamæ. *Spike* nodding, very compact, occasionally branched. *Flowers* connivent, longer than the membranous acuminate bracteæ. *Sepals* strongly marked with distinct, nearly s mple veins; *the upper* lanceolate, compressed, three-nerved, reticulated; *inner lateral* membranous, oblong, with slender simple veins; *lower* narrow, parallel with the lip, at the apex fleshy and deltoid. *Labellum* oblong, unguiculate, entirely membranous, cucullate, wavy, crenate, obsoletely three-lobed; the veins all crested with compressed lamellar interrupted processes, which dwindle towards the margins into little nodosities. *Columna* with a membranous margin, erect, shorter than the labellum.

This has an almost aphyllous habit, and is abundantly distinct from all the other species in its labellum, being wholly membranous, and crested with lamellar appendages.

6. *C. multiflora;* foliis oblongis scapo longè brevioribus, sepalis inferioribus lanceolatis patentibus apice carnosis dilatatis hinc membranaceis, labello oblongo membranaceo unguiculato: cristâ rarâ glandulosâ; lobis lateralibus abbreviatis: intermedio elongato obtuso crispo.

Spontè crescit circa *Valparaiso;* Februario florentem copiosè invenit M^c^Rae, (v. s. sp.)

Stem from a foot to a foot and a half in height. *Leaves* short, oblong

lanceolate, spreading, rising a little way up the stem, which is clothed with long appressed sheathing squamæ. *Spike* oblong, many-flowered. *Flowers* ringent, longer than bracteæ. *Upper sepal* oblong, suddenly acute; *lower* fleshy, and dilated at the end, with the upper margin membranous. *Labellum* ovate-lanceolate, obtuse, cucullate, membranous, scarcely lobed, crenated, wavy, the veins being unequally furnished with small glandular excrescences; *middle lobe* linear obtuse.

7. *C. alaris;* sepalis patentibus: interioribus rhombeis antrorsis intus varicosis; exterioribus reflexis; inferioribus oblongis apice cucullatis carnosis, labello sessili cucullato: lobis lateralibus venosis intermedioque carnoso rotundato glandulosis.

Juxtà *Conception*, Octobre floridam, invenit McRae, (v. s. sp.)

Stem about a foot and a half in height. *Leaves* broad, short, ovate-oblong, not rising up the stem, which is unusually stout, and clothed with long and broad squamæ. *Spike* nearly half the length of the plant. *Flowers* distant, longer than the membranous bracteæ. *Sepals* much spreading; the outer reflexed; the *upper* lanceolate, veiny; *lower* veiny, with a fleshy dilated cochleate apex; *inner*, by a twist of their unguis turned forward, and overlapping each other by their posterior margins, rhomboid-lanceolate, slightly denticulate, painted as it were with varicose anastomosing veins. *Labellum* three-lobed, cucullate, unguiculate, the lateral lobes erect, membranous, crenated, the middle one fleshy and densely crested.

8. *C. bletioides;* foliis oblongo-lanceolatis obtusis, bracteis membranaceis inflatis, sepalis oblongis retusis venosis: inferioribus apice carnosis dentatis, labello subsessili membranaceo cordato, apice obsoletè trilobo: lobis lateralibus membranaceis; intermedio carnoso dentato; cristæ appendicibu spaucis lamelliformibus antice falcatis.

Ad *Cumbre*, Andium claustrum, Novembre floridam, copiosè invenit, McRae; circa *Valparaiso* rarò, Februarii fine (v. s. sp.)

Stem from a foot to a foot and a half in height. *Leaves* numerous, spreading, three or four inches long, obtuse. *Spike* about six inches long, and from six to eight flowered. *Flowers* very large, two-lipped, enveloped in very broad, membranous, acuminate bracteæ, which are a little longer than the ovarium. *Sepals* all of an uniform figure, very distinctly veiny, oblong, rounded at end; the lower fleshy and toothed at end. *Labellum* roundish oblong, slightly stalked, membranous, strongly veined, three-lobed towards the end; the lateral lobes membranous, the middle one longer, rounded, fleshy and toothed towards the middle; crest consisting of a few large lamellæ, tapering towards the back, and falcate in front.

This is by far the finest species, the flowers, including the ovarium, measuring three inches in length. The specimens

found by Mr. M^c^Rae about Valparaiso are more slender in all their parts than those from Cumbre, which is a pass on the Chilian side of the Cordilleras.

9. † *C. virescens.*

Epipactis flore virescente et variegato vulgo Piquichen. *Feuillée Peruv.* 2 *p.* 27. *t.* 19.

Cymbidium virescens; foliis radicalibus lanceolatis; scapo erecto simplici, petalis tribus exterioribus lanceolatis acutis, binis interioribus oblongis obtusis, labello ovato obtuso elevato sulcato. *Willd. sp. pl.* 4. 106.

Hab. in Chile. *Feuillée.*

This appears to be very distinct from any found by Mr. M^c^Rae. It is nearly related to *C. bletioides*, which it seems to emulate in the size of the flowers.

BIPINNULA.

Juss. gen. 65.

Char. gen. Pollinia duo pulverea bipartita, caudiculâ glandulâque nullis. Anthera terminalis opercularis bilocularis . . . Stigma . . . . . Columna semiteres membranaceo-marginata. Labellum anticum cucullatum integrum cristatum appendicibus filiformibus fimbriatum. Sepala ringentia membranacea; supremis nanis; inferioribus labello suppositis, anamorphose solutâ, longissimis fimbriatis. *Herbæ terrestres* (Americæ Australis temperatæ) *radicibus fasciculatis. Folia plana radicalia. Scapi squamosi. Flores colorati?*

1. *B. plumosa;* scapo folioso multifloro, sepalis inferioribus labelloque plumosis.

In pascuis herbidis ad vicum *Colina* urbis *Santiago* finitimum semel vidit M^c^Rae, Novembre floridam. (v. s. sp.)

Stem about a foot and a half high. *Leaves* erect, oblong-lanceolate, somewhat obtuse, gradually changing to long, membranous, acuminate, sheathing squamæ. *Spike* about five inches long, eight or ten flowered. *Bracteæ* membranous, acuminate, nearly as long as the upper sepals. *Flowers* ringent. *Upper sepal* lanceolate acute, *inner* erect, rather shorter, obtuse; *lower sepals* three times as long as the upper ones, ovate-acuminate, spreading, membranous, finely fringed beyond the middle with long delicate filiform processes which arise from the margin. *Labellum* ovate-acuminate, cucullate at base, about one-third the length of the lower sepals, like which it is also bordered with a fringe of long filiform ciliæ. *Columna* erect, half the length of the upper sepals.

Two specimens only of this noble species, one in flower, the other in bud, were discovered by Mr. M^c^Rae in the grassy pastures about the hot-baths of Colina, a small village on the skirts of the plain which surrounds the town of Santiago. The fine fringe that borders the lower sepals and the labellum call

to mind some of the North American species of Habenaria. The flowers measure, from the base of the ovarium to the apex of the lower sepals, fully three inches.

2. *B. Commersonii;* sepalis inferioribus ligulæ formibus apice fimbriatis, caule unifloro (aphyllo ?)

Arethusa biplumata. *Linn. suppl.* 405. *Smith ic. ined. p.* 23. *t.* 23.
Bipinnula Bonariensis. *Spreng. syst.* 3. 745.
Habitat in *Bonaria.* Commerson.

This very obscure plant was originally described by the younger Linnæus from specimens communicated to him by the late Mr. Thouin from Commerson's Herbarium; a figure of it was subsequently published by Sir James Edward Smith. By both these authors the native country was stated to be the Straits of Magellan, an error which was soon corrected by M. de Jussieu who affirms, from the examination of Commerson's original collection, that it was discovered in the neighbourhood of Buenos Ayres. To this Professor Sprengel, upon the very indifferent authority of Molina, adds Chili. But I doubt whether Molina ever saw the plant; it is more probable that he confounded the B. plumosa with this species.

ASARCA.

Char. gen. Pollinia duo pulverea bipartita, granulis triplicibus angulatis, caudiculâ glandulâque nullis. Anthera terminalis opercularis bilocularis. Stigma rotundatum anticum convexum. Columna arcuata libera membranaceo-marginata apice dilatata. Labellum anticum liberum unguiculatum trilobum, membranaceum: lobis lateralibus venosis intermedio elongato obtuso cristato. Sepala subæqualia membranacea venosa; interioribus paulo brevioribus; inferioribus labello suppositis, anamorphose nullâ.—*Herba terrestris* (Americæ Australis temperatæ). *Scapus foliosus non squamosus. Flores colorati ?*

1. *Asarca speciosa.*

Serapias Gavilú. *Pavon. in herb. Lamb.*
Habitat in *Chile.* Pavon. (v. s. sp. in h. Lamb.)

Stem about a foot high, leafy. *Leaves* oblong, sheathing and amplexicaul at the base, becoming smaller by degrees till they assume the habit, but not the texture, of the squamæ of Chloræa and Bipinnula. *Spike* lax, about eight-flowered. *Bracteæ* oblong-lanceolate, shorter than the ovarium. *Sepals* of the same figure, ovate-oblong, acuminate, veiny, in no degree fleshy; the two interior more obtuse and shorter.

The only specimen of this plant which I have seen exists in the Herbarium of Mr. Lambert, where it is marked in Pavon's

writing "Serapias Gavilú de Chile sp. nov. ined." It is probably from the north of Chile, and is very different from the Gavilú of Feuillée, which is a species of Chloræa.

The three genera, Chloræa, Bipinnula, and Asarca, represent the form of Chilian Orchideæ under the three modifications to which it is probable that they are subject. Collectively they constitute a tribe of Arethuseæ with distinct sepals, having an expansion which is always more or less ringent, with a columna having membranous margins, and with a cucullate, crested labellum. Individually they are recognised by the particular state of the lower sepals, and the condition of their parenchymatous matter. Chloræa and Bipinnula are distinguished from Asarca by a tendency in the anterior parts of the flower to anamorphosis, or an excessive production of parenchyma, while Asarca is characterised by the want of such a tendency, except in the labellum, in which anamorphosis exists in no greater degree than in many other Orchideæ. That the peculiarities of the anterior sepals of both Chloræa and Bipinnula depend upon the condition I have stated, may, I think, be easily shown, notwithstanding their apparent difference of anatomical structure. In the species of Chloræa the sepals are entire, and the excess of parenchyma is insufficient to overcome the resistance or power of expansion of the cuticle; while in Bipinnula, in which the sepals are plumose, the anamorphosis is so much increased as to overcome the resistance of the cuticle, and the parenchyma is consequently no longer confined within a narrow space forming a sort of succulent bag, but pushes forth on all sides with nearly equal power, in the form of a membranous feathery fringe of the most beautiful symmetry. That these two forms do in fact depend upon such a power of parenchymatous development as I have assumed, seems to be in some measure proved by this circumstance; that while the anterior sepals are entire (and the power of developement therefore weak), they bear the same proportion to the other parts of the flower as is usual in Orchideæ; but as soon as the anterior sepals become feathery (and the power of developement therefore excessive), they no longer bear the usual proportion to the posterior sepals, but become elongated in the same degree as they become fringed or laterally developed and membranous.

SPIRANTHES.

Richard Orch. ann. p. 28 (1817.) *Bot. Register, fol.* 823.—Gyrostachys. *Pers. syn.* 2. 511 (1807).—Ibidium, *Salisbury in Hort. Trans.* 1. 261 (1812).

1. *S. diuretica;* foliis petiolatis ovato-lanceolatis, scapo apice piloso, spicâ ovato-lanceolatâ congestâ, bracteis florum longitudine, labello sessili undulato crispo venoso: callis linearibus obtusis.

Epipactis floribus uno versu dispositis vulgo Nuil. *Feuillée Peruv.* 2 *p.* 26. *t.* 17.

Neottia diuretica. *Willd. sp. pl.* 4. 73.

Spiranthes Nuil. *Richard. Orch. ann.*

Circà *Valparaiso,* Februario florentem, legit McRae. (v. s. sp.)

General appearance of the plant like that of the Siberian Spiranthes congesta. *Leaves* oblong-lanceolate, stalked, resembling those of Pelexia adnata, but smaller and erect. *Stem* a foot high, clothed with acuminate squamæ, hairy at the upper part. *Spike* crowded, ovate-acuminate, with taper-pointed bracteæ longer than the flowers. *Sepals* downy externally, the three upper of equal length and firmly agglutinated together, the two lower spreading, distinct. *Labellum* erect, sessile, recurved, membranous, crisp, with very strong bifurcate veins, and two calli at the base, which are linear and obtuse, and apparently veiny also. *Columna* unusually short, with a retuse rostellum, and a large roundish cordate anthera.

A Dictionary of Chemistry, and of Mineralogy as connected with it, in which is attempted a complete List of the Names of Substances, according to the present as well as former Systems; with an Introduction, pointing out the order in which the chief parts of the work may be perused, so as to constitute a regular Course of Chemistry; and a Vocabulary, in which the apparatus and processes made use of are briefly described,—with copious Notes, &c. &c. By William Campbell Ottley, *London,* 1827.

This verbose title is, we are sorry to say, the prelude to a very inadequate performance,—to a mere chemical scrap-book. The task of writing a dictionary of chemistry is, at the present day, serious and difficult; and should only be undertaken by those who are well read in the science, and who, with just and accurate information of what has been done by others, conjoin practical skill, experimental dexterity, and much personal observation of the processes of the chemical arts.

What may be Mr. Ottley's talents we know not, nor do we recollect to have met with his name in the annals of chemistry: he has, at all events, on the present occasion, entered upon an undertaking in which he has entirely failed; indeed we cannot even conceive his object in the present publication, which is only fit to keep company with Mr. Accum's work of the same name and nature.

But now that the subject of chemical dictionaries is before us, we cannot let the opportunity slip of giving a word or two of advice to Messrs. Aikins and Dr. Ure, whose works, excellent as far as they go, and in a great measure distinct in their objects and execution, may still admit of improvement.

To Messrs. Aikins we would remark, that chemists are not fond of *quartos;* and we are certain that an edition of their dictionary in *octavo*, executed in the same excellent spirit and style as the former, carefully worked up to the present state of the science, and containing an extension of their practical histories of the chemical arts, would meet with unrivalled success. If we are not mistaken, two closely-printed volumes would embrace every requisite; for we regard the plates of little consequence. Our advice to Dr. Ure is, in his next edition, to get rid of as much more of Mr. Nicholson as he conveniently can: if possible, to put him altogether upon the shelf, and make the work his own. Dr. Ure's original stores of chemical knowledge, the depth of his reading and information, and the perspicuity of his style, are all evilly influenced by the alloy of the old book upon which they are grafted, and which operates upon them as lead upon gold,—for, although it neither alters nor debases the precious metal, it deteriorates and discolours it.

In taking our leave of Mr. William Campbell Ottley, we sincerely advise, that before he again comes forth in the shape of an author, he should seriously consider whether or not he has anything new to communicate, or whether he regards himself peculiarly fitted for conveying known facts, in more intelligible terms and perspicuous language than his predecessors and contemporaries:—in short, whether his stock of chemical knowledge and experience justify his undertaking the compilation of a work above all others requiring them—a *Chemical Dictionary.*

On the best Mode of extracting the Nutritious Portion of Bones, and on its Economical Applications. By M. Charles de Gimbernat.

Fortified places, however well provisioned, are occasionally exposed to scarcity in time of war, either in consequence of the length of the blockade, or from the provisions being spoiled and decayed, or destroyed by bombardment, where the magazines are not bomb-proof. In these cases, it is important that the garrisons should have some means of subsistence without resorting to unwholesome and disgusting substances, in order to prolong the defence; for, where the fate of states depends on the preservation of their fortresses, military annals attest that famine and epidemic diseases produce greater ravages than the bayonet, or even than artillery. Thanks to the progress of science, the effects of these scourges may now be avoided, or at least diminished. The discovery made by Guyton-Morveau on the disinfecting property of chlorine, and certain acids, renders it possible to preserve armies from contagious diseases; and since the Guytonian fumigations have been practised in barracks and military hospitals, mortality from such cause has considerably diminished. But, unfortunately, the preservative against famine recommended by Dr. Papin more than a century ago, has not been hitherto sufficiently attended to: I allude to the use of bones in alimentary economy. True it is that the difficulty of employing steam in extracting the nutritious substance of bones prevented the application of Papin's method; but that is no excuse for the indifference shown by military administrations as to so important an object; for all danger in the use of Papin's digester would long ago have disappeared, had as much attention been given to it as to the boiler of the steam-engine; nor can another serious inconvenience of this process, namely, the decomposition of a great part of the gelatine, occasioned by the high temperature of the steam, excuse the negligence of not having profited by this resource in case of famine. The learned philanthropist, Cadet de Vaux, with a view to remedy the inconvenience just mentioned, proposed to crush bones in the first

instance, and then to extract the gelatine from them by the action of boiling water. The Society for the Encouragement of Industry in France considered this idea sufficiently important to offer a premium for the invention of the best instrument for the purpose, and it was adjudged to Mons. Guerin, of Strasbourg, in 1804. This machine has been employed in several hospitals, as a means of providing the poor with an economical nourishment; but never made use of in fortresses distressed by famine. It is true that experience has not answered the expectations which were formed of this mechanical mode; for the trituration of the bones in considerable quantities heats them, and excites a degree of fermentation, which gives a rank taste to broths prepared with gelatine obtained by this process. All such inconveniences are avoided by the chemical process, which consists in separating the earthy part of the bones by means of muriatic acid, an operation by which they are reduced to a state of a cellular, flexible, and transparent albumino-gelatinous substance, almost perfectly soluble in boiling water. This method has the advantage of affording more nutritive matter, and is preferable from its simplicity, since it requires no extraordinary or expensive apparatus, as where the digester and the crushing-machine are employed. It may be practised without difficulty wherever muriatic acid can be obtained. There is no doubt that this process offers great advantages to the alimentary economy of charitable institutions, and to those arts in which gelatine of a superior quality is required; but, in my opinion, its principal utility consists in offering to military administrations an immense resource, in victualling garrisons when threatened with dearth or famine. With this view I proposed and practised it at Strasbourg in December, 1813. At that period, so critical for the safety of France, Strasbourg was very insufficiently provisioned, the sudden reverses of the French being totally unexpected: on the invasion of the French territory, Mons. de Lezay-Marnesia, Préfet of the Lower Rhine, anxious to put the chief place of his department in a condition to withstand a long siege, and not having the means of obtaining provisions in sufficient quantity, consulted several persons on the best mode of employing

bones as a means of subsistence for the garrison and inhabitants. The processes of Papin and Cadet de Vaux were proposed, the only ones then known: the second was more particularly recommended; for as there was at Strasbourg the crushing-machine, invented in that town, the trituration of bones was more practicable than the process of softening them by steam, an operation which could not conveniently have been practised upon a large scale. The experiment did not answer, the broth thus obtained having a rancid taste, for the reason above-mentioned. Being at that time at Strasbourg, I was asked by the Préfet if some other means could not be found of accomplishing the object? I answered, that it might be accomplished without difficulty, by dilute muriatic acid and boiling water. The Préfet, pleased with this idea, desired me immediately to make the experiment. I did so. My success was complete; two days afterwards, soup made from the animal matter of bones, extracted by this process, was served at his table, and highly approved. In consequence of this, immediate measures were taken to obtain an abundant supply of bones from the slaughter-houses and kitchens. I did everything in my power to second the zeal of the Préfet, and remained at Strasbourg, in order to co-operate with him in protecting the inhabitants and the garrison from disease and famine, the only enemies they had to fear. By degrees the disinclination of the people to this form of food was overcome. The process, of which I published the details, grew into general use; and the soups and jellies so made, when properly seasoned and flavoured, were acknowledged to be of an unexceptionable quality.

As Strasbourg was blockaded at the beginning of January, 1814, a short time only after the plan of bones had been suggested, the quantity necessary for the relief of a garrison could not be procured; but all which could be obtained were carefully employed during the blockade, for the nourishment of the poor, who ate this kind of broth without repugnance, and liked it better than the aqueous soups of Count Rumford: thus a great quantity of meat and other provisions were daily economised. The blockade was fortunately raised before the scarcity was felt; these measures of precaution, therefore, became

superfluous for the moment, but they ought not to be overlooked in future.

It was natural to presume that an experiment so important would not be lost, but that it would attract the attention of the military administrations, especially as the process was practised at Paris as well as Strasbourg, whilst all communication between the two towns was interrupted. But as soon as the danger had disappeared it was no longer thought of. I attribute the neglect into which it has fallen principally to the death of the Préfet, who acknowledged its importance, and intended to have furnished the fortresses of his department in peace-time with supplies of bones and acid. When Strasbourg was again threatened, in 1815, I returned there, and made the same proposal to the Duke of Albufera, who commanded the military division of the Rhine. The Marshal expressed his intention of profiting by it, but a few days afterwards he gave up the command: on that occasion his principal staff-officer, Baron de St. Cyr-Nugues, inserted in the *Journal of Military Medicine* for 1815, printed in Paris, an account of what had been done at Strasbourg in 1814. This was the first public announcement of the advantages of my process. This process was afterwards recommended by M. Michelot, in the *Révue Encyclopédique* for 1822, vol. xiii.

Still I am not aware that any country has adopted this precaution, so easy of adoption and so important. I attribute this neglect partly to habitual, but dangerous, indifference to perils which are not immediate, and partly to the opposition of private interests. A supply of bones and acid, which are not liable to damage, holds out no temptation to speculators, and little profit to commissaries. The French government, in 1817, began to supply their colonial garrisons and their navy with gelatine prepared in the manufactory of Robert and Co. in Paris; but this application, however good, is not what I recommend—*viz.* the supplying fortresses, with gelatine, not prepared by art, but gelatine contained in bones in their natural state, together with a proportionate quantity of acid. This recommendation is founded on this important circumstance, that the mineral part of bones is the best preserver of their animal substance. Bones are, therefore, an important resource against

famine; because, as long as they are kept in their natural state, a food as nutritious as fresh meat may be obtained from them. For this reason, a supply of bones is preferable to cakes and other preparations of gelatine, which cannot be so perfectly preserved, and are always liable to the attacks of vermin. It has been ascertained that even bones, which are commonly reputed antediluvial, furnish an unexceptionable jelly. We may, therefore, safely assume, that bones kept for ages will furnish man with wholesome food, and that a large supply of these will be a security against famine.

The subsistence of garrisons might always be provided for, if there were in the fortresses a sufficient quantity of bones; which might easily be procured in time of peace, and almost without expense, for all that would be necessary would be to preserve the bones left after the daily consumption of meat; and an order, which prescribed to the garrisons such a measure, would render the necessity of capitulating for want of provisions less frequent. In a few years bones enough would be accumulated to feed several thousand men for a considerable time; the quantity thus collected, indeed, would be immense; but there would be no difficulty or expense in preserving them: it is not necessary to have magazines for an article, which can be kept equally well above, or under ground, provided it be secured from excessive humidity. The bones might be buried in the dry ditches of the ramparts between two beds of sand, together with a proportionate provision of acid.

Here, then, is a security against famine, which would at the same time be proof against weather, shells, fire, and the ravages of time. It is scarcely possible to have food better preserved, than by this process, which might not improperly be called a *para-famine*, by the French Academy, since they allow the terms *para-tonnerres* and *para-grêles*.

Recent events sufficiently show the importance of making regulations for the preservation of the bones of the meat consumed in fortresses. The garrisons of St. Jean d'Ulloa and of Callao made a most honourable defence; but famine at length forced them to deliver up these places, the last possessions of Spanish power, to the republics of Mexico and Peru. By their surrender, Spanish dominion has terminated in the

vast continent of the New World; and Missolonghi surrendered under similar circumstances, notwithstanding the heroism of its defenders.

Though the process which I have recommended for extracting gelatine is generally the best, circumstances may render the old method necessary; and fortresses should consequently be provided with crushing-machines and digesters. The chemical process, for instance, would be impracticable where the acid could not be obtained; neither could it be used without abundance of water, for the purpose of washing away the acid from the animal texture, which is an operation indispensably necessary.

In order to avoid all misconception, I beg distinctly to state, that what I propose is not intended as a substitute for other and better provisions, but merely as a precautionary sustenance, to which garrisons may have recourse when other means are exhausted. It has been objected, that the gelatine of bones is insipid, and, consequently, disagreeable; but it may be rendered savoury by means of different substances, which are easily procured, such as salt, spices, and vegetables. It must also be remembered, that man, exposed to famine, is not so particular with regard to his nourishment; as is proved by many instances of garrisons, which have been fed on substances the most insipid, unpalatable, and disgusting. As a proof of this, a circumstance may be mentioned, related to me by General Rapp, which took place during the last siege of Dantzic:—that the French, when they had eaten horses, rats, and dogs, had recourse at last to broth made of old planks,—a dish, not merely unpalatable, but devoid of all nutritious matter.

The remaining portion of M. de Gimbernat's communication, which will appear in our ensuing number, is dedicated to the friends of the Greeks. It impresses the necessity of the utmost economy of provisions, and points out several important applications of the process above described, by which the author hopes that the surrender of fortresses, in consequence of famine, may become a circumstance of comparatively rare occurrence.

Hints respecting the Construction of Warm-Baths.

Warm Baths have lately come into very general use, and they are justly considered as indispensably necessary in all modern houses of any magnitude, as also in club-houses, hotels, and hospitals. But the mode of constructing these baths, and of obtaining the necessary supplies of hot and cold water, does not appear to me to have undergone an improvement equal to the extension of their employment; I trust, therefore, you will not consider a few pages of your journal misapplied to a subject, which I think materially affects the health and comfort of the community, and upon which a variety of erroneous and conflicting opinions are extant.

The several points which I propose briefly to touch upon, in regard to warm baths, relate,—

1. To the materials of which they are constructed,
2. To their situation,
3. To the supply of cold water,
4. To the supply of hot water,
5. To minor comforts and conveniences.

1. As to the materials of which they are constructed.—Of these the best are slabs of polished marble, properly bedded with good water-tight cement, in a seasoned wooden case, and neatly and carefully united at their respective edges. These, when originally well-constructed, form a durable, pleasant, and agreeable-looking bath; but the expense is often objectionable, and in upper chambers the weight may prove inconvenient. If of white or veined marble, they are also apt to get yellow or discoloured by frequent use, and cannot easily be cleansed; so that large Dutch tiles, as they are called, or square pieces of white earthenware, are sometimes substituted, which, however, are difficultly kept water-tight; so that, upon the whole, we recommend marble.

Where there are reasons for excluding marble, copper or tinned iron plate are the usual materials resorted to. The former is most expensive at the outset, but far more durable than the latter, which is, moreover, liable to leakage at the

joints, unless most excellently made. Either the one or the other should be well covered outside and in, with several coats of paint, which may then be marbled, or otherwise ornamented, as taste suggests.

Wooden tubs, square or oblong, and oval, are sometimes used for warm baths, and are cheap and convenient, but neither elegant nor cleanly: the wood always contracts a mouldy smell; and the difficulty and nuisance of keeping them water-tight, and preventing shrinkage, is such as to exclude them from all except extemporaneous application.

2. As to the situation of the bath; or the part of the house in which it is to be placed.—In hotels and club-houses, this is a question easily determined: several baths are usually here required, and each should have annexed to it a properly-warmed dressing-room. Whether they are up stairs or down stairs is usually a mere question of convenience, but the basement story, in which they are sometimes placed, should always be avoided; there is a coldness and dampness belonging to it, in almost all weathers, which is not agreeable.

In hospitals, there should be at least two or three baths on each side of the house (the men's and women's), and the supply of hot water should be ready at a moment's notice. The rooms in which the baths are placed should be light, and comparatively large and airy, and such conveniences for getting into and out of the bath should be adopted as the sick are well known to require. The dimensions of the baths should also be larger than usual.

In private houses, the fittest places for warm baths are dressing-rooms annexed to the principal bed-rooms; or where such convenience can be obtained, a separate bath-room, connected with the dressing-room, and always upon the bed-room floor. All newly-built houses should be properly arranged for this purpose, and due attention should be paid to the warming of the bath-room, which ought also to be properly ventilated. A temperature of 70° should be easily kept up in it, and sufficient ventilation is absolutely requisite, to prevent the deposition of moisture upon the walls and furniture.

The objection which formerly prevailed, in respect to the difficulty of obtaining adequate supplies of water in the upper rooms, has been entirely obviated, by having cisterns at or near

the top of the house; and we would just hint that these should be so contrived as to be placed out of the reach of frost—a provision of the utmost importance in every point of view, and very easily effected in a newly-built house, though it unfortunately happens that architects usually regard these matters as trifles, and treat them with neglect, as indeed they do the warming and ventilation of buildings generally.

3. The supply of water, of proper quality and quantity, is a very important point, as connected with the present subject. The water should be soft, clean, and pure, and as free as possible from all substances mechanically suspended in it. In many cases, it answers to dig a well for the exclusive supply of a large house with water: in most parts of London, this may effectually be accomplished, at a comparatively moderate expense; and if deep enough, the water will be abundant, soft, and pellucid. The labour of forcing it by a pump to the top of the house is the only drawback; this, however, is very easily done by a horse-engine; or there are people enough about town glad to undertake it at a shilling a-day. I am led to these remarks by observing the filthy state of the water usually supplied, at very extravagant rates, by the water companies. It often partakes more of the appearance of pea-soup than of the pure element; fills our cisterns and pipes with mud and dirt; and even when cleared by subsidence is extremely unpalatable. It deposits its nastiness in the pipes connected with warm baths, and throws down a slippery deposite upon the bottom of the vessel itself, to such an extent as often to preclude its being used, at least as a luxury, which a clear and clean bath really is. This inconvenience may in some measure be avoided, by suffering the water to throw down its extraneous matters upon the bottom of the cistern, and drawing our supplies from pipes a little above it; there will, however, always be more or less deposite in the pipes themselves, and every time the water runs into the cistern the grouts are stirred up, and diffused through its mass: this, from some cause or other, with which I am unacquainted, has lately become a most intolerable grievance; and he who reflects upon the miscellaneous contents of Thames water, will not have his appetite sharpened by a draught of the Grand Junction beverage, nor feel reanimated and refreshed

by bathing in a compound so heterogeneous and unsavoury. Let us, however, to cut this matter short, suppose that we have plenty of pure cold water to deal with, and proceed to our most essential topic, namely, the best mode of heating it in the quantity required.

4. and 5. In public bathing-establishments, where numerous and constant baths are required, the simplest and most effective means of obtaining hot water for their supply consists in drawing it directly into the baths from a large boiler, placed somewhere above their level. This boiler should be supplied with proper feeding pipes and gauges; and above all things, its dimensions should be ample: it should be of wrought-iron or copper, except where sea-water is used, in which case the latter metal is sometimes objectionable; but my remarks are at present intended to apply to fresh-water baths only. The hot water should enter the bath by a pipe at least an inch and a half in diameter, and the cold water by one of the same dimension, or somewhat larger, so that the bath may not be long in filling. The relative proportions of the hot and cold water are, of course, to be adjusted by a thermometer; and every bath should have a two-inch waste pipe, opening about eight inches from the top of the bath, and suffering the excess of water freely to run off, so that when a person is immersed in the bath, or when the supplies of water are accidentally left open, there may be no danger of an overflow.

Where there is a laundry in the upper story of the house, or other convenient place for erecting a copper and its appurtenances, a plan similar to the above may often be conveniently adopted in private houses for the supply of a bath upon the principal bed-room floor. An attempt is sometimes made to place boilers behind the fires of dressing-rooms, or otherwise to erect them in the room itself, for the purpose of supplying warm water—but this plan is always objectionable, from the complexity of the means by which the supply of water is furnished to the boiler, and often dangerous from the flues becoming choaked with soot and taking fire; steam is also apt in such cases to escape in quantities into the room, so that it becomes necessary in many cases to search for other methods

of heating the bath, one or two of the least objectionable of which I shall describe.

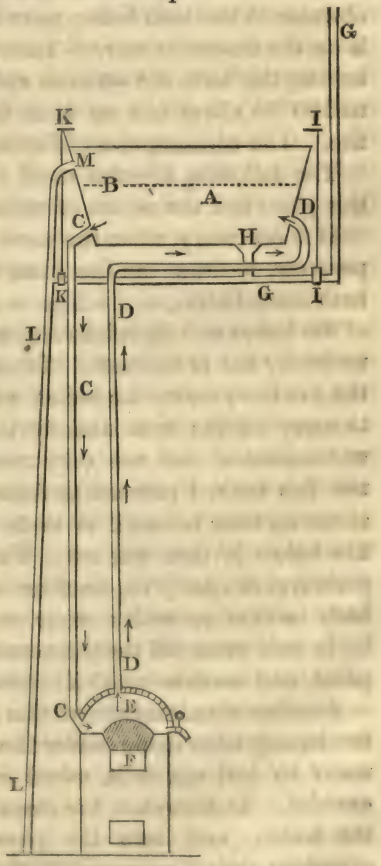
1. A contrivance of some ingenuity consists in suffering the water for the supply of the bath to flow from a cistern above it, through a leaden pipe of about one inch diameter, which is conducted into the kitchen or other convenient place, where a large boiler for the supply of hot water is required; the bath-pipe is immersed in this boiler, in which it makes many convolutions, and, again emerging, ascends to the bath. The operation is simply this:—the cold water passing through the convolutions of that part of the pipe which is *immersed in the boiling water*, receives there sufficient heat for the purpose required, and is delivered in that state by the ascending pipe into the bath, which is also supplied with cold water and waste pipes as usual. The pipe may be of lead, as far as the descending and ascending parts are concerned; but the portion forming the worm or convolutions immersed in the boiler, should be as thin as is consistent with security, in order that the water within it may receive heat without impediment; it may therefore be made of copper.

This plan is only plausible and economical where a large boiler is constantly kept at work in the lower part of the house; otherwise the trouble and expense of heating such a boiler, for the mere purpose of the bath, render it unavailable. The worm-pipe is also apt to become furred upon the outside by the deposition of the earthy impurities of the water in which it is immersed; it then becomes a bad conductor of heat, is cleansed with difficulty, and the plan is rendered ineffective. This system, however, has been adopted in some particular cases with satisfaction and success.

2. A much more simple, economical, and independent mode of heating a warm-bath by a fire placed at a distance from it is the following, which I shall describe in detail, as it is found to answer perfectly in private houses, as well as upon a more extended scale in large establishments. It is certainly open to some objections, but these are overbalanced by its preponderating advantages. A waggon-shaped boiler, holding about six gallons of water, is properly placed over a small fur-

nace in any convenient and safe part of the house,—the kitchen, scullery, servants'-hall, or wash-house may be used for the purpose. The bath itself, of the usual dimensions and construction, is placed where it is wanted, with a due supply of cold water from above: two pipes issue from within an inch of the bottom of the bath at its opposite extremities; one at the head of the bath about one inch, and the other at the foot an inch and one-eighth in diameter. These tubes descend to the boiler, the smaller one entering it at the bottom, and the larger one issuing from it as it were at the top.

Under these circumstances, supposing the pipes and boiler every where perfectly tight, when the bath is filled the water will descend into and expel the air from the boiler, and completely fill it. Now, upon making a gentle fire under the boiler, an ascending current of warm water will necessarily pass upwards through the larger pipe issuing from its top, and cold water will descend by the pipe which enters at bottom; and in this way, by the establishment of currents, the mass of water in the bath will become heated to the desired point; or if above it, it may easily be attained by the admixture of cold water.



The annexed sectional wood-cut will, perhaps, render this description more intelligible. A is the bath. B the level of the water in it. C the descending pipe of cold water. D the ascending pipe of hot water. E the copper boiler with its fire-

place. F G is the pipe admitting cold water into the bath by the opening H, when the stopcock I is open, and K closed; but when I is closed, and K open, the same pipe serves to empty the bath, the water flowing off through the general waste-pipe L, which also carries away the overflowings of the bath, through M.

The advantages of this form of bath are numerous. The shorter the pipes of communication from the bath to the boiler the better, but they may extend forty or fifty feet without any inconvenience beyond that of expense: so that there is no obstacle to the bath being near the bed-room while the boiler is on the basement story. There is but little time required for heating the bath, the water in which may, if requisite, be raised to 100° in about half an hour from the time of lighting the fire. The consumption of fuel is also trifling.

The following are the chief disadvantages attendant upon this plan, and the means of obviating them:—

It is necessary, when the water has acquired its proper temperature, to draw the fire from the boiler, and not to use the bath immediately, as it will go on acquiring some heat from that of the boiler and its brickwork, so that we may become inconveniently hot in the bath. To obviate the necessity of raking the fire from under the boiler, a flue may be so constructed as to carry off the heat directly into the chimney; but this is uneconomical and not very convenient. When, therefore, I use this bath, I proceed as follows:—I heat the water in it, about an hour before it is to be used, to about 100°, and the fire below is then put out. The water will retain its temperature, or nearly so, for three or four hours, especially if the bath be shut up with a cover, so that when about to use it, I let in cold water till the temperature is lowered to the required point, and we thus avoid all the above inconveniences.

Another disadvantage of this bath arises from too fierce a fire having been made under the boiler, so as to occasion the water to boil within it, *which ought always to be carefully avoided.* In that case, the steam rising to the upper part of the boiler, and into the pipe D, condenses there, and occasions violent concussions, the noise of which often alarms the whole house, and leads to apprehensions of an explosion,

which, however, is very unlikely to occur; but the concussions thus produced injure the pipes, and may render them leaky: so that in regard to these, and all other baths, &c. we may remark, that the pipes should pass up and down in such parts of the house as will not be injured if some leaking takes place; and under the bath itself should be a sufficiently large leaden tray with a waste-pipe, to receive and carry off any accidental drippings, which might injure the ceilings of the rooms below. In all newly-built houses, two or three flues should be left in proper places for the passage of ascending and descending water-pipes; and these flues should, in some way, receive at their lower part a little warm air in winter, to prevent the pipes freezing: the same attention should also be paid to the situation of the cisterns of water in houses, which should be kept within the house, and always supplied with a *very ample* waste-pipe, to prevent the danger of overflow. Cisterns thus properly placed, and carefully constructed, should be supplied from the water-mains by pipes kept underground till they enter the house, and not carried across the area, or immediately under the pavement, where they are liable to freeze.

3. Baths are sometimes heated by steam, which has several advantages: it may either be condensed directly into the water of the bath; or, if the bath be of copper or tinned iron, it may be conducted into a casing upon its outside, usually called a jacket; in the latter case there must be a proper vent for the condensed water, and for the escape of air and waste steam. Steam is also sometimes passed through a serpentine pipe, placed at the bottom of the bath; but none of these methods are to be recommended for adoption in private houses, and are only advisable in hospitals, or establishments where steam-boilers are worked for other purposes than the mere heating of baths. A description of the different modes of employing steam as a means of heating masses of water for baths and other uses, would lead me into too wide a field of discussion for the present paper, which, therefore, I shall here conclude.

C.

A Manual of Chemistry, chiefly for the Use of Pupils of Mechanics' Institutions. By Andrew Fyfe, M.D., F.R.S.E., &c. *Edinburgh*, 1826.

This little book scarcely deserves the name which the author has conferred upon it,—it is not a handful, but a mere *pinch* of chemistry; and though it may answer very well as a text-book for his course of lectures, cannot be commended as a work of reference, or as holding out any temptations to the general scientific reader. It is, moreover, disfigured by a set of villainous wood cuts, to which the doctor, however, seems passionately attached; for he repeats the same block several successive times, where mere reference to it would have been sufficient.—Thus, at page 15, is a most clumsy representation of a furnace, with a tube passing through it, for the decomposition of vapours; and this ugly thing is reiterated at pages 109, 121, 129, 141, 165 (without the retort, but the same cut), and lastly, at page 197: and an equally unintelligible portrait of a retort and gas-receiver occurs at pages 5, 14, 17, 122, 151. This, in a little book of 300 pages, is too bad.

But Dr. Fyfe's information is not always correct, especially upon points of importance and interest to those whom his Manual immediately addresses. Hops, for instance, are described as the *seed-pods* of a plant much cultivated in England; and at page 244, we are told that the brewing porter is carried on in the same way as that of ale and beer, *with this difference*, that the malt is prepared in a peculiar way. "It has been already said, that that used for ale is dried by the application of a slight heat. In making malt for porter, a much higher temperature is applied, by which it is slightly burned, so that the wort got from it has a dark colour, and a peculiar bitter taste." We believe few porter-brewers would grow rich if they thus employed *slightly-burned* malt. But lastly, Dr. Fyfe says, that "gin, or Hollands, is always prepared by the Dutch:" this may possibly do for an Edinburgh audience, especially after the elaborate account of "whiskey" which precedes the above quotation; our London mechanics, however, know better; few of them, as we apprehend, being unacquainted with the merits of Hodges' *full proof*.

Upon the whole, however, though there are many marks of haste, and some of carelessness, visible in this compilation, we are ready to admit that it is not ill adapted for a text-

book to the classes it is intended for. The arrangement is well enough; and when the deficiencies are filled up by experimental illustrations, or verbal descriptions, we dare say that it will answer the purpose of enabling the student to draw up, at his leisure, more copious notes and perspicuous illustrations than those with which Dr. Fyfe has supplied him.

On the Structure and Habits of the Seal. By John Harwood, M.D., F.L.S., Professor of Natural History in the Royal Institution.

[This communication contains the outline of a discourse on the above subject, delivered to the members of the Royal Institution, by Dr. Harwood, on Friday evening, the 16th of February, 1827. It was illustrated by numerous specimens of skeletons, and of stuffed animals from the Museum of the Royal Institution, and from the valuable collection of Mr. Brookes.]

I HAVE principally been induced by two motives to select the Seal as the subject of the present discourse: the one is, that it has not attracted that general and popular notice with which its habits and peculiarities deserve to be viewed; and the other, that throughout the whole extent of animal life we discern no more beautiful and obvious adaptation in structure to those peculiarities and habits than are presented to us in the anatomy of this creature; indeed, these striking appropriations are so numerous, that it becomes difficult to make choice of those most worthy of attention. Externally, the seal is of an elongated form, its neck powerfully muscular and long, and its body formed like that of a fish, broadest across the chest, and gradually tapering to its hinder extremities. For the convenience of swimming, its fore limbs are so short, as to appear destitute of some of the bones and parts found in those of land-quadrupeds: for we only see externally the feet, having their toes provided with sharp claws; but they are so enveloped in broad membranes as not to be readily traced, though capable of free motion. Such a capability, however, only applies to our northern seals, and a few other species; for the eared kinds of the South Seas have their toes almost immovable, and furnished with flat nails on the fore feet. After the gradual tapering of the body, which terminates in a short flattened tail, the hinder

feet are observed to be furnished with still broader membranes, which in many South Sea species are even extended beyond the ends of the claws, whereby the extent of surface is greatly increased; but in all, aided by the powerful muscles of the spine, these feet act with immense force on the surrounding fluid, and produce an extremely rapid progression. From such external characters, a beautiful connexion is at once observed between the seal and those aquatic animals which surround it, its feet, for example, being intermediate in their structure between the simply webbed ones of the otter and beaver, and the flattened fin-like ones of the manati, the whales, and other cetacious mammalia which are more exclusively adapted to a watery element. But in the skeleton of the seal, these gradations and adaptations are still more apparent: though composed of the usual number of bones, the length and flexibility of its neck is of the highest importance in its economy; for, by the slightest inclination of the head, at the end of this long lever, in any direction, while diving, its centre of gravity becomes instantly changed at its will; and thus are its submarine chases, even after the swift salmon, rendered so marvellously successful, that its only mode of escape consists in darting into the shallows. In the general form of the skeleton, seals bear no very distant relation to the weasels, the chest having an unusual extent of motion, by the free articulation of its vertebræ, and, as in those animals, the liver and lungs are each divided into several distinct lobes, that they may glide smoothly over each other, and not oppose the great curvature to which their bodies are liable. On the same principle, their ribs are placed farther asunder than in most others; while the lumbar regions and pelvis, as in all diving animals, are long and narrow, for the attachment of powerful muscles. We now see that the limbs, although so curiously shortened for aquatic operations, possess the same number and arrangement of bones as those of animals whose actions are terrestrial, subject, however, to interesting modifications; for instance, in the fore-feet, the thumb or inner toe is the strongest, and the outer the weakest; but in the hinder feet, to increase the force and extent of membranous surface opposed to the water, the two outer toes are far the longest and strongest.

From such an aquatic conformation, then, it is sufficiently obvious that the movements of the seal on the land are necessarily slow and imperfect: they have been, not unaptly, compared to those of a caterpillar, being chiefly effected by vertical flexures of the spine. Nevertheless, even under these disadvantages, seals defend themselves and their young with their teeth, with great courage and address; thus I recollect having seen a seaman who had been most severely wounded from too daringly attacking a large seal. It was observed by Aristotle, that most animals have their fore teeth sharp and their inward teeth broad; but that the seal has them all pointed; now, in fact, in the different species of seal, the skull not only affords much variety in its form, but their teeth differ so greatly from each other, that nothing would be more easy, were it by any means desirable, than to substitute new generic names for almost every known species. These differences are well exemplified in the various specimens before you, in which we observe their front teeth to vary in number, having either four or six above and four below, while their molares vary from sixteen to twenty-four, all of which, in common with their canine teeth, have more or less pointed surfaces.

In the Northern seals, the front teeth above have single points; but in the antarctic species, the four middle, upper, and, indeed, lower front teeth have a double edge, the two outer ones having single points: their molares are either simply conical, like those of the common seal, or are each armed with three points as in the Phoca Grœnlandica.

As sub-aquatic pursuits were those destined for the seal, and as these were liable to be often accompanied with labour and difficulty, it is obvious that they could not be carried on by animals internally organized like those of the land; for the necessity for so frequently rising to the surface to breathe, would have been an effectual impediment to their success. The Creator has, therefore, so modified the mode of circulation in the seal, that this inconvenience has been counteracted, and yet this has not been effected as in the reptiles; for in them the vessels of the heart are so constructed, that the blood can flow freely through them, without going to the lungs, and thereby occasioning a necessity for breathing; nor (which

would produce the same effect) do the auricles of the seal communicate, as was formerly thought. On the contrary, in the seal the original type in the construction of the heart is still retained, as also in the walrus and otter, and other aquatic mammalia, with this exception,—that the veins which return the blood to the heart are so much enlarged, that they are capable of changing their office, and becoming reservoirs for receiving and retaining the blood in its progress to the heart. Thus is the right side of the latter, and the lungs, prevented from being oppressed by its superabundance when the creature is under water, and incapable of breathing, and thus is its life sustained. The largest of these venous reservoirs exists in the liver of the seal; but its whole venous system, like that of the walrus and the whales, is very greatly developed. If I may be allowed the expression, like most other aquatic mammalia, the seal appears to be literally gorged with blood; its blood is moreover of an unusually dark colour, being almost black, which property it perhaps acquires by its constant liability to become arrested in its course; and hence, perhaps, the necessity for so much blood in its circulating system—yet the animal heat of the seal is very great.

We shall next briefly advert to some of the *senses* of this animal. Its very large and dark eyes being directed more forwards than in any other aquatic quadruped, added to the round and human appearance of its head, when raised above the surface of the water, doubtless caused it to contribute greatly towards the formation of those ideal marine monsters of which the ancients have favoured us with so many accounts. The eye of the seal is provided with a most perfect membrana nictitans, or winking membrane, the use of which it is rather difficult to conceive in aquatic animals, except to shield their delicate organs from the too powerful effects of the light, in rapidly rising from the depths to the surface. The pupil of the seal is vertical, like that of the cat; but its soft expressive physiognomy, which more nearly resembles that of the dog than any other quadruped, and is equally expressive of superior intelligence, is not affected by the cat-like form of the pupil, in consequence of the dark colour of the iris. But the greatest peculiarity in the construction of its eye, is a narrow zone, or

two zones, as in the Phoca monachus of the Grecian Islands, encircling the globe, of a thinner texture, so much so as to be only one-fourth part of the usual thickness, and more flexible than the rest of the sclerotic coat; and as the straight and oblique muscles of the globe are inserted anteriorly to these flexible zones into a thicker part of the tunic, it is probable that their simultaneous contraction may alter the length of the axis of the eye, and the form of the transparent cornea, by approaching the latter to the retina, and by rendering it more or less convex, and thus better adapt it to the different media in which the creature lives. I think this the more likely, because I have often observed that seals, on first appearing on the surface of the water, appear somewhat bewildered, and do not distinctly discern objects, till their eyes have had time to adapt themselves to the more rare medium to which they are exposed.

The nose of the seal is an organ of more perfect formation than that of any other quadruped. The nostrils are most accurately closed at the entrance by very perfect valves, to prevent the ingress of water when it dives, and, indeed, at all other times, except when it respires. Its breathing also, at all times, occurs at very irregular intervals, often extending to half a minute between each inspiration, but the quantity of air it then receives is very great. As to the internal formation of the nose, it possesses one of the most beautiful structures which the whole class exhibits, especially from the amazing number of the convolutions of those bones on which the infinite ramifications of the olfactory nerves are spread. It has, indeed, been computed by Sir Busic Harwood, that the smelling surface in the nose of a single seal amounts to the enormous extent of two hundred and forty square inches. Now it is no less worthy of remark that something of this curious complication in the organ of smelling likewise obtains in other aquatic animals, and especially the otter, which is a very remarkable circumstance, when we consider that, as before observed, their nostrils, like their ears, are most accurately closed by valves, to prevent the entrance of the water when they dive, and, indeed, at all other times, except during breathing. A question, therefore, naturally arises,—that if, in the pursuit of their prey, or other sub-

aquatic actions, they are at all governed by this greatly-developed faculty, in what way are odorous impressions conveyed to the nose?—an inquiry which has, I think, been by no means satisfactorily explained by those who suppose odorous impressions to be conveyed to the nose posteriorly, from the entrance of the throat.

I really suspect, however visionary it may appear, that seals hunt their prey, or discover its vicinity, by their sense of smelling, when swimming on the surface of the water; for if dogs are very sensible to the scent of a small aquatic fowl on the surface, as we know is the case, why should not the seal of a distant shoal of fishes, sporting, as they constantly do, on the surface, its proper prey, and by means of organs so far more complicated? The same view of the subject of course equally applies to the delicate nasal organs of the otter and the Polar bear. Vision, then, I would say, is the sense which governs the seal in its actions beneath—smelling, when upon the surface, and only when out of the water.

The seals of the Northern seas are almost entirely destitute of outward ears. The opening into the organ is, like that of the nose, accurately closed by means of a perfect valve. The internal organ of hearing in the seal presents another beautiful intervening gradation in structure between that of the entirely aquatic and the entirely terrestrial quadrupeds: like the latter, the ossicula are attached to the membrana tympani; and like the true whales, the inferior circumference of the tympanum consists of bone, of unusual thickness and solidity. As to the faculty, I have reason to believe that seals not only hear acutely, but they are sensibly gratified by musical sounds. When I was most amongst these animals, they much excited our amusement; for, by uttering a whistling sound, we could readily induce them to follow our boats very great distances, when they would continue to raise their heads above the surface nearer and nearer, and to fix their large eyes stedfastly upon us.

The tongue of the seal is notched at its extremity, unlike that of most other quadrupeds: it is perfectly smooth, yet is doubtless provided with a tolerably acute sense of taste. The predilection of this animal for the salmon is manifest, from the cir-

cumstance of that fish tempting it further within the reach of human persecution than any other; hence, I have often seen seals rise to the surface to breathe with salmon in their mouths, at that period of the year, when they lie in such numbers around the mouths of Irish and Scotch rivers, to cut off the retreat of that fish into the sea, after depositing its eggs in fresh water.

The seal's throat is so provided with a valve at its entrance, that the creature has no difficulty in swallowing its prey when under water, without admitting the latter into the stomach.

Of all aquatic genera of animals, the seal appears to be the most widely distributed over the surface of the globe. The common kind, although generally an inhabitant of temperate seas, not only excites much interest by its annual and well-conducted migrations from shore to shore, but often swims northwards into very frigid regions, though seldom higher than the 78th degree of latitude, where it associates with the Phocæ Grœnlandica, cristata, and barbata.

If we regard the antarctic regions, we find them no less the abode and resort of seals than the arctic, as has been lately signally evinced by the hundreds of thousands which were discovered on that vast tract of desert land south of Cape Horn, which has been called South Shetland, and which commences about the 70th degree of latitude, and from whence they have already almost been extirpated by unceasing persecution. From these seas Lord Anson first brought to Europe the immense Phoca Leonina, the Sea Lion, or Sea Elephant, as it is more frequently called, from its possessing a short trunk, and being a creature of no less than 20 feet in length, and the largest species known. Lord Anson was also, I believe, the first who described the poisonous effects of the liver of the South Sea seals; and I have been informed by a Greenland Captain, that he himself, and one or two others suffered very severely from partaking of the kidney of the common seal. The Phoca leonina, closing the extremity of its short trunk, greatly inflates it when enraged; so likewise, in the North seas, the Phoca cristata, or bladder-nosed seal, has a kind of hood on the head, which it can inflate with air, and with it protect its eyes and nose when attacked: it measures eight feet in length, and is very ferocious. But the largest seal of our own seas, is the Phoca barbata, the species

which I think goes by the name of ground seal among the Greenland fishers, and of whose skin the Greenlanders are said to make their thongs for fishing. A specimen of this seal was shot on the North coast of Scotland, which measured twelve feet in length. There is, I believe, one of these preserved on the top of the staircase at the British Museum.

Among the seals, which are provided with external ears, of the South Seas, and which have been associated under a new name, are the Phoca jubata and the Phoca ursina. The former being provided with a mane, and measuring fifteen feet long; while the latter is of much smaller size, and is destitute of a mane.

I have been much surprised at the numbers of common seals, from three to five feet in length, which frequent the Western coast of Ireland, having often seen there more than twenty reposing together on a rock. Their numbers appear not only to be owing to the unfrequented nature of that interesting part of Great Britain; but certain superstitions of the fishermen induce them on all possible occasions to spare their lives. The common seal also abounds on the North shores of Scotland, and the Hebrides; but, of course, from increasing persecution, rapidly decreases in numbers as we proceed south.

The brain of this animal is, I think, doubtless, of greater proportionate magnitude, than in any other quadruped, and not only does it exhibit in its countenance the appearance of sagacity, but its intelligence is in reality far greater than in most land quadrupeds: hence its domestication is rendered much easier than that of other animals, and it is susceptible of more powerful attachment. These circumstances do not excite more interest among the naturalists of the present day, than they did long ago with Aristotle, Ælian, Pliny, and other ancient observers of nature. It is evidently the common seal, to which Aristotle alludes in his description; and his observation, that it is the only quadruped which searches for its food in the sea, would lead us to suppose that he had not an opportunity of acquainting himself with the walrus and sea otter.

The large seal, which was exhibited some time ago at Exeter Change, appeared to me to understand the language of its keeper as perfectly as the most faithful dog. When he entered at one end of its long apartment, it raised its body from the water,

in which it was injudiciously too constantly kept, supporting itself erect against the bars of its inclosure, and wheresoever he moved, keeping its large, dark eyes stedfastly fixed upon him. When desired to make obeisance to visitors, it quickly threw itself on one side, and struck the opposite one several times in quick succession with its fore foot, producing a loud noise. The young seal again, which was kept on board the Alexander, in one of the Northern expeditions, became so much attached to its new mode of life, that after being thrown into the sea, and it had become tired of swimming at liberty, it regularly returned to the side of the boat, to be retaken on board. Such examples might be greatly multiplied, but these are amply sufficient for our purpose; and I cannot help stating, that aware of this disposition to become familiar, this participation in the good qualities of the dog, it is astonishing that mankind have not chosen this intelligent and finely-organised quadruped, for aquatic services scarcely less important than some of those in which the dog is employed, on the surface of the land.

The seal is among the few polygamous quadrupeds; and like the rest, the males, during the period of intercourse, enter into violent conflicts. Two young ones are generally produced at a time in the autumn. They are brought forth in caverns, extending from the sea above high-water mark, and here they remain suckling during several weeks, before they venture into the water. When they become fatigued by swimming, we are assured that the parent supports them on her back.

The voice of the seal consists of a kind of bleating, whence doubtless has the common seal obtained the name of the sea-calf, the Phoca vitulina: the voice is, however, liable to great variation, especially during changes in the state of the atmosphere, when they become extremely clamorous. Spending their nights on the surfaces of rocks, or upon the shore, it is during their very sound repose in these situations that they are approached and destroyed with sticks; and when thus alarmed, they tumble their young ones before them into the sea, and are themselves often sacrificed in the very act. But the means by which they are captured in by far the greatest numbers, is by cutting off their retreat to the sea at low water, when, in apparent security, they lie in large herds within

caverns: fire-arms, in these cases, are never had recourse to, as a comparatively slight blow by a stick on the head, or nose, is sufficient to destroy life: this arises either from the thin and fragile nature of the bones of the skull of the seal, or the great and immediate nervous communication which exists between its nose and its brain. Among more uncivilised nations, its mode of destruction is far more tedious and less efficient. Thus, for instance, the Esquimaux, after long watching, first strikes a seal with his barbed spear, to which a line is attached, having at its extremity a large floating buoy, composed of the inflated skin of another seal; thus entangled and opposed in all its efforts to retreat far beneath, as often as it rises to breathe, it receives a fresh wound from the unerring spear of its destroyer.

We now proceed to say a few words on the uses to which this animal is applied, when deprived of life, by the more civilised nations of mankind. As to the benefits which the inhabitants of frigid regions derive from it, they are far too numerous and diversified to be particularized, as they supply them with almost all the conveniences of life. We, on the contrary, so persecute this animal, as to destroy hundreds of thousands annually, for the sake of the pure and transparent oil with which it abounds; 2dly, for its tanned skin, which is appropriated to various purposes by different modes of preparation; and, 3rdly, we pursue it for its close and dense attire. In the common seal, the hair of the adult is of one uniform kind, so thickly arranged and imbued with oil, as to effectually resist the action of the water; while, on the contrary, in the antarctic seals the hair is of two kinds: the longest, like that of the northern seals; the other, a delicate, soft fur, growing between the roots of the former, close to the surface of the skin, and not seen externally; and this beautiful fur constitutes an article of very increasing importance in commerce;—but not only does the clothing of the seal vary materially in colour, fineness, and commercial estimation, in the different species, but not less so in reference to the age of the animal. The young of most kinds are usually of a very light colour, or entirely white, and are altogether destitute of true hair, having this substituted by a long and particularly soft fur.

On Artificial Cobwebs for Micrometers.

By C. R. Goring, M.D.

A COMMON cobweb has always appeared to me to be a very strong substance in proportion to its extreme tenuity, for a bulky spider will frequently depend from a single fibre of great length:—small insects are incapable of overcoming the resistance of the meshes of a web;—which will also endure for a considerable time in wet and windy weather without requiring much repair. Nevertheless, I have always found astronomers extremely nervous about the threads of their micrometers, which, it is said, may be destroyed by the trifling current of air which passes through the tube of a telescope when its objective and eye-glasses are displaced; probably because the cobwebs, being of animal origin, are subject to decomposition in course of time, for I do not think a cobweb, fresh from the body of a spider, could be so easily affected. Be this as it may, I have accidentally discovered a mode of making artificial ones, which are not subject to decomposition, which may be stretched by mechanical violence to double their length without snapping, returning again to their original dimensions—which may be blown upon by a powerful pair of bellows, with its nose close to them, without injury, and which can be rendered of any degree of tenuity and fineness, being a perfect substitute for the ordinary ones, as it seems to me; but as I am not a connoisseur in such matters, it must be left to Messrs. Troughton and Sims, who have certainly arrived at the extreme verge of improvement in the construction of micrometers, to determine the point at issue between me and the spider. The following is the receipt for making them:—Procure some of the purest essential oil of turpentine, shave some caoutchouc extremely thin, and put it into a small wide-mouthed phial, observing not to fill it more than one quarter; pour in the turpentine to the top, and secure it carefully with a cork and bladder. Let this be worn in the pocket for a few days, and the India rubber will absorb the essential oil, and become greatly increased in bulk; then let a portion of it be put into another bottle with some more turpentine, and in a few days it will be completely dissolved. It is necessary that there should be more turpentine employed in the first instance than

is required to reduce the India rubber to the consistence necessary to form cobwebs, in order to allow impurities to subside to the bottom of the phial, and to insure the complete solution of the caoutchouc, for if any particles of it are left untouched, smooth even cobwebs will not be obtained*. This solution must be kept about the person till a good deal of the turpentine has evaporated, to aid which process it will be advisable to have the phial merely closed by a piece of paper tied over it: the older it is the better. If any heat is employed to effect the solution, exceeding that of the human body, the consequence will be, that the caoutchouc will undergo a change, and will never dry or return its original state. When the India rubber in the phial has assumed a viscid consistence, like that of birdlime, it is in a fit state for making cobwebs: the precise condition in which it is most fit for that purpose may be known by experiment, and by its sticking two other pieces of India rubber together in such a manner, that they cannot be separated when the interposed cement is dry.

The making of the cobwebs is the most simple affair imaginable; all that is necessary is to take a small quantity of the solution on the point of a bit of wood, and to stick it to a frame, producing the viscid thread, which will proceed from it to the opposite side: in this way any quantity may be made. This

* There will always be the same difficulty in obtaining truly parallel and even threads from the caoutchouc as there is from common cobwebs. I have one of Messrs. Troughton and Sims's best micrometers by me at this moment; and the cobwebs, though, of course, picked and abundantly even enough for practical purposes, are certainly far from being of the same diameter throughout. I suspect, from microscopical examination, that a common cobweb is a flat thread like a piece of tape, not a round one, so that, if it is at all twisted, it must present different apparent diameters to the eye. The best way of getting parallel threads with the India rubber paste, appears to me to be to let it stand for a considerable time without any agitation, or stirring up whatever, and when the instrument is inserted to draw out a thread, to disturb the matter as little as possible, touching only its surface; the more the thread is drawn out the more even and true it becomes. It may not be irrelevant to mention, that in order to give the fibres their utmost degree of strength, they should be put on in the lowest degree of tension possible; in drawing them out on the frame, therefore, let the instrument recoil as much as it will before the end of the thread is fastened down, that at least the caoutchouc may not be stretched till its ultimate attachment to the micrometer is completed. Rectified white naphtha may be tried instead of the turpentine.

process should be carried on in a very warm room, otherwise the liquid thread is apt to snap. The fibres thus prepared should be carefully preserved from dust for a day or two, till completely dry; they may then be examined by a microscope, and the most even and parallel selected. In this way we obtain artificial cobwebs, having the same opacity, and the same power of enduring the solar spectrum as common ones, and which appear to me to bear as much rough usage, damp, &c. as can reasonably be desired in such delicate fibres; for when the turpentine has evaporated, they are left neither more nor less *than unchanged caoutchouc.*

On the Importance of preserving Facts connected with the Natural History of Animals. By William Swainson, Esq., F.R.S., F.L.S., *&c.*

The chief reasons why the study of Natural History in this country is less popular than others and much less attractive, appears to be the prevalent notion that it is now become a science too vast and too difficult for general cultivation;—that no sooner is one system established, than it begins to give place to another:—that to discover the scientific cause of any one object, expensive libraries must be consulted, or national museums referred to;—that the student is unassisted by that public instruction which is afforded him in every other department of science;—that its application to the comforts or conveniences of life is remote or indirect;—and finally, that the natural history of those animals we are daily in the habit of seeing, is too well known to require any very particular attention.

With the hope of exciting more general attention to this subject than it at present receives it will be my object, in this sketch, to show that many of these objections are magnified, and that others are without real foundation.

To take such a comprehensive view of this, or any other science, as will sensibly extend the boundaries of knowledge, and remove its present land-marks, is, indeed, an eminence which few, under the most favourable circumstances, can hope

to attain. The greatest talents are of little avail, without the use of a costly library and a well-stored museum. Such advantages will be seldom found; and therefore the higher departments of the science can only be prosecuted by those few who possess them, and who devote their lives to this single object.

When we look to the numerous changes that have taken place in systems during the last ten years, and find that another revolution is now going on, we may at first be tempted to question its necessity, and to complain that so much time has been lost in acquiring that which must be unlearnt. But a moment's reflection will show the unreasonableness of such a conclusion. Natural History, like every other branch of human knowledge, advances by slow but steady paces. Had Dr. Johnson lived in the nineteenth century, he would not have applied the epithet *adamant* to any system but that of nature; which, although "unchanging and unchanged," can hardly be said to be yet discovered. The truth is, that zoology is now undergoing a similar revolution to that which chemistry experienced some years ago. Nothing but facts are taken upon trust. The authority of a name, however great, or the plausibility of a system, however ingenious, avails nothing when unsupported by such tests; so that we now begin with doubting the solidity of that basis upon which our predecessors built what they considered as improvements. In short, Natural History is a study of facts; and it is the right combination of these facts which constitutes the true philosophy of the science. It may reasonably be supposed that, having at length chosen nature for our guide, we shall make some advance towards the temple of truth; that when once those obstacles which have impeded our progress are cleared away, a system and a nomenclature will be established, not indeed perfect, but sufficiently so to remain in use, with little alteration, for the rest of our lives.

Yet admitting this supposition to its full extent, it may still be said, that only a minor difficulty has been removed. A library and museum are still necessary; for, without such aids, how can we hope to investigate any portion of the eighty thousand species of living beings already described, the thousands of others that are known to exist, and the tens of thousands which,

in all probability, remain to be discovered? True. But the objection is, in part, groundless, because it implies the practicability of doing that which, under any circumstances, is next to impossible. No naturalist of the present day, however enthusiastic, ever dreams of investigating every species in his cabinet, for the mere object of giving them names. In proportion to the magnitude of his subject, or to the extent of his collection, he limits his inquiries. He is content to acquire a knowledge of generic types,—of the species of any particular group, or of those only which belong to one country. It is true that his studies will not be attended with that variety which results from a more general survey of nature; but the real interest, by such a restriction, is rather increased: and, by confining his attention to a smaller number of objects, he may feel assured of understanding them, in the end, much better, and of being able to illustrate their history with more truth, than any of his predecessors might have done. Fewer points of reference, either from books or specimens, are necessary. And the means of adding something to the general store of knowledge is thus placed within the power of every one who can provide himself with a few elementary works. Trembley immortalised his name by discovering the reproductive powers of the polype*; and the anatomical treatise of Lyonnet, who fixed his attention on a single insect, has been pointed out as a model of perfection.

Nevertheless, so varied are the channels by which information is to be sought for, and so numerous are the paths which centre in the portico of the temple of Nature, that the most unscientific observer has the power of contributing something to illustrate the economy of creation. Natural History cannot be learned from books, or from museums: these are, indeed, necessary to acquire what is termed a *systematic* acquaintance with the subject, and so far may be considered essential. Yet this constitutes but a small and comparatively uninteresting portion of the science. The true naturalist does not content himself with making a tour of museums,—with naming their contents, with manufacturing names, or with framing ingenious theories from stuffed skins: he goes beyond the precincts

* A. Trembley, immortel par la découverte de la reproduction du polype. *Cuvier. Règ. Anim.* iv. p. 165.

of his closet to view Nature in her true garb, and to contemplate the innumerable ways by which CREATIVE WISDOM has provided for the wants, the pleasures, and the protection of "every living thing that moveth upon the earth." Woods and mountains, plains and valleys, each peopled by their own inhabitants, afford him never-failing instruction and delight;—they impart a freshness and interest to his writings, which not only stamps them with a permanent value, but renders them interesting, if not captivating, to all cultivated minds. There are few who have not perused the "Natural History of Selbourne," a collection of letters on the facts observed by its philosophic author, respecting the animals of his native village *. Mr. White was a country clergyman, who held little intercourse with the world, and was almost unknown to the *scientific*. In truth, he seems to have given little of his attention to system, and still less to learned societies. Yet, with all these disadvantages against a short-lived popularity, what book, ever published in this country, has rendered Natural History more attractive, or has, in consequence, been more generally read? We feel interested in the little anecdotes this true philosopher has preserved of every subject that crossed his path; and almost fancy we hear him narrate the incidents of his evening's walk, while seated in his own garden hermitage. His style is suited to his subject—easy, chaste, and natural. The absence of *system*, in point of fact, is the great charm of the book; and *this* will give it an interest when most of its learned brethren are consigned to oblivion. But this is not all. The observations of such a man afford a perpetual source of information to naturalists of the highest order. In our attempts to grasp the general system of nature, we must thread all those labyrinths by which she passes, with so much subtlety, from one form to another,—from genus to genus, and family to family,—till at last, ascending to the two great types of animal and vegetable life, she blends† them so imperceptibly, that the most philosophic obser-

* The Natural History of Selbourne. London, 1822. 2 vols. 8vo., the last edition.

† In the observations of the learned Agard on the red snow, of which my friend Professor Hooker has given an interesting abstract, is the following passage:—"Hence it follows, that this substance must be either an alga or an animalcula, between which I know of no limits; there are

vers have been unable to draw between them a line of separation.

Now, to trace these innumerable threads of affinity, so far as the economy of animated beings is concerned, we must have recourse, upon every occasion, to the practical naturalist. I have mentioned White, as being the first of our countrymen who enriched the science with these genuine records of nature; but this is not the only instance. No one has thrown more light upon the habits and economy of the ornithological portion of our British Fauna than the late Colonel Montague. His writings in general, and his supplementary volume to the Ornithological Dictionary in particular, afford a mine of information to the philosophic naturalist, and of interest and amusement to the general reader. He studied nature; and, in recording what he saw, thought it not beneath him to point out that one bird hopped while another walked; or that the nest of another was built in a low bush, instead of being placed upon a high tree. By persons accustomed to look upon systems and learned names as teaching everything, these humble anecdotes will be smiled upon with contempt: but by those who have entered deep into the study of nature, they will be prized as facts, trivial, indeed, in themselves, but assuming an importance little dreamed of by their narrators, when coupled with others, and interwoven in the great scheme of Nature.

Were we, in fact, to single out all those naturalists of the last century whose writings are of real value and authority at the present day, we should find the list (with a few exceptions) filled by the names of those who have drawn their observations from nature, and made system a very secondary consideration. The merits of the Baron de Geer * were long obscured, nay

forms, then, which may, with equal propriety, be ranked with either or both. There are algæ which become animalculæ, and *vice versâ*. Lastly, there are infusoria, which, at one period of their existence, are endowed with the power of movement, while in another they exist only in the character of a vegetable." *Brewster's Journal*, No. 7.

* An independent spirit, which prompted De Geer to refuse obsequious submission to the authority even of Linnæus, and an utter absence of any partiality for nomenclature, seem, with a strong natural feeling for the observation and arrangements of facts, to have been the principal causes of the excellency of his works. *M'Leay*, *Horæ Entomologicæ*, vol. i. part 1. xxvii.

utterly unknown, in this country, for many years, so completely was true science lost sight of in an overweening attachment to Linnæan and Fabrician nomenclature. In later times, the extensive observations made by Le Vaillant on the birds of Africa, and the still more delightful volumes of Wilson on those of America, have invested this department with an interest it never before possessed.

But enough has been said to prove, first, that a very important and a very interesting department of the science can be prosecuted without the aid of either libraries or collections, or without the study of systems or names. Secondly, that the most minute details in the economy of animals should not be overlooked as trivial or unimportant. And, thirdly, that those who have pursued this path have conferred lasting benefits upon natural science.

It is unnecessary for me, in this place, to expatiate upon the peculiar interest that attaches to natural history, even when pursued as a mere recreation, or without any view of benefiting others by our observations: the interest which it gives to a summer's walk or to a turn in the garden, the habits of quick observation which it excites in the youthful mind, or the expansion it gives to the heart in more mature life,—all these, and many more of its attractions, have been enlarged and illustrated by abler pens than mine. The study of Nature is opposed to the study of man, chiefly in this respect; that the first presents everything that is pure, lovely, and "*of good report*," while the latter is continually bringing before us fraud, duplicity, violence, and all those passions which, without being imbibed, insensibly blunt the better feelings of our nature.

But if all these arguments should fail, and people will only be induced to take an interest in things which affect their immediate comforts, we shall find that some acquaintance with natural history is not only useful, but sometimes essential to the direct interests of a large portion of the community. There is no one class in whose success the interests of mankind are so much involved as in that of the agricultural. By this I mean, that, as food is the first necessary of life, every question which concerns its production deserves very serious attention. Now it is well known, that every year some unexpected failure of crops, origi-

nating in the depredations of the insect world, takes place; that the labours of the farmer, and the hopes of the planter, are continually destroyed by these minute and subtle enemies; and that a local scarcity, and often individual ruin, is generally the result. With these evils upon record, and continually coming under our notice, in one form or other, every year, any one would fancy that that portion of natural history, at least, which related to agriculture, planting, and gardening, had been well studied; that books had been written upon the subject; that the history and economy of all these insects had been well investigated, and that effectual means had been pointed out for counteracting their injuries. But what is the true state of the case? Incredible as it may appear, this subject has been almost neglected. No book professing to give the agriculturist a right knowledge of the animals useful or injurious to his labours, exists in our language. By what unaccountable chance the various agricultural societies have overlooked this matter I know not, nor is it my present intention to inquire. This, however, may be safely asserted, that it deserves to be made the subject of contention for the greatest prize they have it in their power to bestow. That I may not be supposed to attach to this part of natural history more importance than it really deserves, I shall merely refer the reader to several facts noticed by Mr. Kirby in his Introduction to Entomology, vol. i. letter 6. But for the good sense of government, in calling in the aid of the most consummate naturalist in Europe, every elm tree in the royal parks round London would, before this time, have been destroyed*. Fortunately, in this latter case, the nature of the injury, and the whole economy of the insect, could be studied on the spot. But it generally happens, that when a naturalist is applied to for information on such points by those who are the immediate sufferers, and he begins to put questions which can alone enable him to form any opinion, he can seldom make out whether the insect is a beetle, a fly, or a moth. He is told, "it may have only two wings, or perhaps four;"—or "it looks like a *grub*." If we turn again to books on gardening, how vague, and sometimes how absurd, are the general directions for preserving fruit

* See Mr. W. S. M'Leay's paper "On the Insect which has destroyed the Elms in St. James's and Hyde Parks," in Edinb. Ph. Journal, No. 20.

trees from "*the* grub" and "*the* caterpillar;" as if all grubs and all caterpillars were alike—appeared at the same season—and were to be destroyed by the same means! It is even lamentable to see even respectable writers treat this subject in much the same vague, loose sort of way. In this, as in medicine, the disease or injury must be watched from its commencement through all its stages; accurate observations must be noted down, even on the most trivial point; and finally, if the injury does *really* originate in an insect, specimens of that insect, in all its stages of growth, must be preserved. With such materials the naturalist's advice may be asked with some advantage. But until the parties most interested will take this trouble upon themselves, no reasonable hope can be entertained of curing the evil.

To this general ignorance of the true nature of the most common animals, those, in fact, which we are daily in the habit of seeing, must be attributed the prejudice which most farmers, and many country gentlemen, entertain against crows and rooks —birds which nature has kindly given us, to keep within due limits the myriads of insects, which would otherwise devastate and destroy our crops. But enough has been said to show how usefully a slight knowledge of natural history may be applied to subjects immediately affecting our interests or our comforts; and that so far from there being nothing left to record in the history of our native animals, there is yet a wide and inexhaustible field open to those who are disposed to illustrate the subject, either for their own gratification, or for the benefit of mankind.

Every admirer of nature must, however, rejoice to see that natural history has assumed an importance in this country, within the last few years, which it has hitherto never been thought to possess, and that its cultivation is now spreading with a zeal and a rapidity quite unexampled. To increase this feeling, by showing that the science may be successfully prosecuted by all who have either leisure or inclination, and that there are paths open to the most humble admirers, has been the object of these pages. Nor shall I consider that time unprofitably employed which has been given to a subject possessing, as this assuredly does, so much general interest.

Warwick, Feb. 25, 1827.

Outlines of Human Physiology, by Herbert Mayo, *Surgeon, &c. &c.* London, 1827.

Under this title, Mr. Mayo has published a system of Physiology, distinguished no less by the clearness and perspicuity of its style, than by the sound and judicious views of the human economy which it presents. Accustomed to deliver lectures on Anatomy and Physiology, Mr. Mayo, has learnt to state, at once lucidly and concisely, the body of knowledge which has been long accumulated; and the advantage which he possesses as a practical physiologist, enables him to digest no less ably the facts included in more recent discoveries, to which he has himself contributed.

The different parts of the subject are treated in the following order:—The properties of the blood; the nature of muscular action; the forces which circulate the blood; the phenomena of the pulmonary circulation; and the use of the flow of arterial blood through the body.

Digestion is next considered;—the production of chyle; the elimination of the refuse matter: the former subject leading to the history of the lacteal and lymphatic vessels; the latter to a description of the kidneys, and of the skin, taken as excretory organs.

The functions of the brain and nerves, of the organs of sense, and voluntary motion, are described afterwards.

The volume closes with a description of the function of generation, and of the phenomena attending the growth and reparation of parts.

The account of the circulation of the blood is, perhaps, the cleverest part of the work, considering the great diversity of interesting questions which it essentially involves.

Upon the nature of the irritability of the heart, Mr. Mayo remarks: "the alternate action and relaxation of the muscular fibres of the heart appear not, like similar phenomena in voluntary muscles, to depend upon a series of impressions transmitted from the brain or spinal chord. If the heart be taken from the body of an animal immediately after death,—if the blood be carefully washed from its internal surface,—if, at the transverse furrow, the auricles be separated from the ventricles by a clean incision, the alternate states of action and relaxation continue to succeed each other in each part as before. For the brief period during which it is reasonable to suppose that the heart retains its perfect organization, no stimulus seems required to excite it

to contract. The alternations of action and repose seem natural to it, or to be the result of its structnre. It is remarkable that if the heart, yet beating, be placed in warm water, it continues to act more briskly, and for a longer time, than if exposed to the air; but that if water be injected into its blood-vessels, its flesh becomes pale and swollen, and after two or three beats hardens permanently: if its fibres be transversely cut through, their action is stopped at once."

The causes which combine to give motion to the blood are summed up in the following manner.

"During the systole of the ventricles, blood expelled from their cavities and thrown into the arteries, urges on that before contained in either arterial trunk, and in its branches, in the capillary vessels, and in the veins, towards the auricle of the opposite side of the heart. During the succeeding diastole many forces continue to operate, which tend to diminish the area of the vascular system, and as the blood is prevented returning into the ventricles by the semilunar valves, they serve to propel it towards the auricles. These forces consist in the elasticity of the arteries themselves, in the compression of surrounding elastic organs, in the contraction of muscular parts, in the pressure of the atmosphere.

"Various causes combine to give effect to these forces in filling the auricles with blood, which operate by taking off or diminishing the atmospheric pressure upon their outer surface. Each auricle during its diastole spontaneously expands: the elasticity of the lungs constantly tends to draw apart the walls of the auricles; and at the time of each inspiration, while the area of the chest is enlarging, the heart is probably relieved of external atmospheric pressure in the same manner as the lungs, although in a less degree."

The theory of absorption is a theme of warm discussion up to the present moment.

The ancients believed, that the veins exercised the function of absorption. In the beginning of the seventeenth century, Asellius saw chyle in a separate and peculiar set of vessels in the mesentery; these vessels were termed lacteals, and admitted to be the instruments intended for the removal of chyle; while absorption in other instances continued to be attributed to the veins. In the latter part of the last century, Dr. Hunter made out the universality of the distribution of lymphatic vessels throughout the body, and was led by analogy to suppose that they were the pro-

per organs of absorption, not the veins; and his opinion was confirmed by experiments made by Mr. John Hunter, which went to show, that the lacteals would absorb other substances besides the chyle, from the cavity of the intestines.

But the recent experiments of MM. Flandrin, Majendie, Ségalas, and Fodéré, have sufficiently proved that there was some error in the experiments of Hunter; that the lacteals absorb chyle alone; and that every other substance, which makes its way from the cavity of the intestines, or from a wound, into the system, directly enters the blood-vessels

In repeating the Hunterian experiment, Mr. Mayo discovered the fallacy which probably misled its distinguished author. It appears that the lacteals, when empty, and seen against an obscure medium, are of a blue colour. If a portion of mesentery be examined, when the lacteals are full of chyle, and they happen then to empty themselves, what were before white lines, on expelling their contents, become blue. But it happened that the fluid which Mr. Hunter employed in almost all his experiments was a solution of starch and indigo: he thought he saw the blue fluid absorbed; when probably he only saw the vessel emptied of its usual white contents.

M. Majendie supposes that his experiments prove not only that the lacteals are employed exclusively in absorbing chyle, but that in every other instance, where absorption takes place, the veins alone are concerned.

A little reflection, however, as Mr. Mayo makes it appear, is sufficient to show, that no such sweeping inference can be drawn from the data which we possess; and that, on the other hand, we have good reason to suppose that the blood-vessels are *in no instance* absorbents, properly so called; the conclusiveness of M. Majendie's experiments being set aside, by supposing the introduction of poisons into the blood-vessels to be the simple result of inert transudation, or to be of the same nature with the diffusion of the bile after death, through the textures adjacent to the gall-bladder.

Mr. Mayo proceeds further to argue in favour of the reasonableness of Dr. Hunter's supposition, that the lymphatic vessels are the agents employed upon intersitial absorption.

When the flesh for a small extent has imbibed an acrid virus, the poison of lues for instance, an ulcer takes place, or, in other words, the poisoned part is absorbed;—in this

instance the lymphatic system, not the vascular system, becomes irritated, as if affected by transmitting an acrid substance.

When, as life advances, the substance of the lungs becomes mottled with black matter, the bronchial lymphatic glands put on a similar appearance: at the same time the lymphatics in other parts do not change their colour; and the vessels, which pass through the blackened bronchial glands, are not the bronchial veins, but the lymphatics from the lungs.

In examining the functions of the nervous system, Mr. Mayo proposes as a subject for preliminary inquiry, What are the moral phenomena common to human beings with the lowest animals?

"We attribute consciousness to animals from the resemblance of their habits to our own. We form definite ideas respecting the nature and extent of their moral properties, by explaining their habits upon the simplest principles, which produce the like in us.

"The perception of a sensation is one of the simplest affections of consciousness that we can conceive. Upon analyzing this phenomenon, we distinguish three elements contained in it: a material impression is communicated to an organ sense, a change ensues in the physical condition of the nervous system, and perception follows. Thus, in vision, rays of light impinge upon the retina, an affection of the optic nerve and brain ensues, and we see light or colour. Of the three parts into which this phenomenon resolves itself, the last alone is an affection of consciousness; the two first are affections of matter, the antecedence of which distinguishes perception from mental phenomena, that resemble it. The conception or imagination of objects in a dream is as vivid and persuasive of their reality as actual perception, but is not like the latter produced by impressions upon our organs of sense.

"The term sensation is legitimately used in two senses; either to denote the physical change in our organs which precedes perception, or as synonymous with the term perception. By blending these two ideas, sensation is frequently understood to signify an affection of the mind resulting from material impressions, which is supposed to be the proper object of perception; but the element thus assumed is a logical fiction.

"We can imagine a corporeal being, the moral properties of which should be limited to sensation only; it is possible,

for instance, that plants feel, although we have every reason to believe the contrary to be the fact. In this and in similar cases, however, we have no means of ascertaining the existence of sensibility. Movements in other beings, such as in ourselves are voluntary, constitute our only evidence that they feel.

"Volition is another simple affection of consciousness, to define the nature of which we have but to abstract from it the phenomena usually presented to the mind in combination with it. We that are blessed with health habitually connect volition with a sense of muscular effort; but when the limbs are paralysed, the patient looks at them and wills in vain their movement; with him the act of volition, like the apprehension of an humorous conceit, or any other purely intellectual operation, is unattended with sensation. Volition, in the sense to which it is commonly limited by physiologists, means but *the attempt* to produce muscular action; and the affection of the mind, though, during health, habitually followed by this result, may take place perfectly, as happens on various occasions, including the instance above selected, without a single muscular fibre moving.

"Besides sensation and volition, a third element is wanted to compose a scheme of consciousness analogous to our own. Some cause must be supposed capable of determining the occasions upon which the will energizes: *motives* must exist to actuate the will.

"Upon referring to the ordinary operations of our own minds, volition appears to take place, whenever we have reason to anticipate a certain degree of gratification or advantage from its exertion. We know, by experience, the prompt influence of the will upon our muscular frame:—we are able to conjecture, with more or less certainty, the consequences of different voluntary actions; and we *will*, with a general or precise understanding of what the result will be, and in order to obtain it. A hungry person knows that the food he prepares to eat will gratify his appetite; a drowning person hopes that his cries will bring people to his assistance. But there are other instances in which conscious motives can be assigned for voluntary actions. The infant at the breast, or struggling when first plunged into water, employs efforts for its sustenance or preservation *no less voluntary* than those already instanced. But in the infant, the motive which leads to the voluntary effort is not a calculation of advantage, but a spontaneous tendency, a blind inclination, an instinct.

"Instinct appears to consist in a natural tendency to the performance of definite voluntary movements upon the occurrence of certain sensations or states of inward feeling. We have experience of its influence in our own persons at every period of life, and can analyze its nature, both under circumstances where it does not admit of controul, and in cases where we are capable of modifying or repressing its impulses."

Having thus defined the simplest elements of a form of consciousness analogous to our own, Mr. Mayo proceeds to show, upon the concurring evidence of comparative anatomy, of experiments made upon animals, and authentic histories of acephalous children, that the parts of the human nervous system essentially and exclusively connected with sensation, volition, and the commonest instincts, are the nerves, with the spinal marrow and medulla oblongata. We have not room to extract the testimony upon which the preceding position rests, nor to follow the numerous interesting inquiries into which the subject branches. Let us limit ourselves upon this head to a few remarks upon the account of instinct, which Mr. Mayo adopts.

To consider instinct as a motive actuating the will, not as a principle analogous to volition, and capable of superseding it, has the recommendation of simplicity, and serves better than other hypotheses to explain consistently several remarkable cases. A singing-bird reared from the egg in absolute solitude, or precluded from hearing the song of any other bird, acquires self-taught the note belonging to its kind. On the other hand, a singing-bird brought up exclusively with birds of another species, learns the song of that species without acquiring that of its own race. The tendency to imitation is a remarkable characteristic of all animals during the early part of life; and we seem plausibly and consistently to account for the modification of the instinct here, by stating, that the pleasure attending the imitation of the song of its fellows formed a stronger motive with the bird, than the innate tendency to practise the notes of its kind.

Animals are led by instinct to select particular haunts; if they have been frequently disturbed they desert these haunts, for others naturally less eligible; in this, and similar instances, the recollection of the alarm repeatedly experienced, forms a motive for refraining from what the instinct prompts.

The countenance naturally bears a definite expression for every shade of feeling; but when most excited, many men

can, by an effort, preserve a perfect serenity of features. In certain states of mind, we are instinctively led to throw certain muscles into action; but suppose that we feel the expediency of concealing our emotions, upon this stronger motive we give the features a different character.

Habitually we breathe with a certain degree of frequency upon instinct; in other words, we draw our breath not upon a specific calculation of what we shall gain by it, but from an innate tendency to do so; but if we have an adequate object proposed, we breathe at a different rate: and if we have breathed for a short time by a conscious effort of the will with measured inspirations, we allow the function to return to its usual rate without any abrupt transition, as from one mode of action to a different mode.

It further deserves to be considered, that although some parts may more commonly act upon instinct than others, yet no distinction exists in the muscular frame between parts that act on instinctive impulses, and parts which act only on measured and conscious instigations of the will. The muscles of the face commonly play instinctively, but during violent emotion the whole frame partakes of the same unconscious expression.

And what is perhaps still more conclusive,—in those parts, the muscles of which act alternately upon instinct or otherwise, one nerve alone regulates equally each mode of action. Mr. Mayo mentions having ascertained, upon dividing the portio dura of the seventh nerve on each side of the head of an ass, that the muscles of its face were absolutely and entirely paralyzed for every kind of movement.

In the account of the special senses, the phenomena of vision are elaborately explained. As extracts from this chapter, we shall give Mr. Mayo's account of the *rationale* of vision by inverted images, and of the nature of the action of the iris.

Mr. Mayo quotes the following experiments made by Scheiner from Dr. Reid.

1. If the head of a pin strongly illuminated be held at the distance of four inches from the eye, the object appears large and imperfectly defined. If a card be then interposed, pierced with a pinhole, the object when seen through the pinhole appears distinct at the same distance. If the interposed card be pierced with two pinholes a line asunder, two objects are seen instead of one; and on closing one pinhole, the object *referred to the opposite side* vanishes. In other words, when an object is painted upon two distinct points

of the retina, it appears double, and the direction to which it is referred has a determinate relation to the place on which its image falls.

2. If the head of a pin strongly illuminated be viewed at the distance of eighteen inches, through a card perforated with two pinholes, it appears not double but single; and its apparent place does not shift on closing one or other of the pinholes.

In other words, the direction in which rays of light fall upon the retina has no influence in determining the direction in which we see the given point of an object; or it is a law in the constitution of the eye, that we refer all impressions made upon a given point upon the retina to a visual object seen in one and the same direction; that direction appears to be a line vertical to the point of the retina, on which the impression is made. Thus, when we produce an appearance of coloured circles, by pressure upon the retina above, below, internally, or at the outer part, the spectrum in succession shifts its place, being always seen in a direction exactly opposite or vertical to the point where the pressure is made.

Education, therefore, which teaches us the real magnitude and true distance of objects, has nothing to do with forming our notions of their true direction from us, or of their visual magnitude.

With reference to the second point adverted to, Mr. Mayo remarks, "that the readiest manner of accounting for the alteration of the size of the pupil is to suppose the substance which forms the unattached margin of the iris irritable. In many instances the iris distinctly consists of two portions, which appear from their colour to be differently organized, of an outer broader part, and an inner narrow ring. In birds especially, in which the pupil is as mobile as their vision is perfect, the inner ring of the iris generally presents a hue totally different from the outer, beginning at an abrupt line. In the ring-necked parroquet, the inner ring of the iris, which has a grey colour, becomes sensibly narrower when the pupil contracts.

"Upon watching the eye of a cat or hawk, the contraction of the pupil appears often to be voluntary. When the eye of the animal is bent upon an object that excites its attention, yet which does not shift its position, the pupil may be seen to enlarge and contract alternately. The animal is probably employed in examining the object under different lights, by intentionally admitting more or fewer rays through the pupil.

"Another remarkable circumstance concurs with the preceding, in establishing a resemblance between the action of the iris and that of the voluntary muscles. The iris receives nerves from two sources, from the sentient part of the fifth, and from the third: the main part of the latter is distributed as a voluntary nerve to the muscles of the eye. Now, if the head of a pigeon be cut off, and instantly after the upper part of the cranium be removed, and the entire brain be taken out, on pinching the portion of the third nerve, which remains attached to the eye, the iris acts suddenly, just as the biceps flexor cubiti acts in an animal recently killed, when the nerve which supplies that muscle is pinched. A similar injury produced no visible effect.

"If, again, the third nerve be divided in the cranial cavity, while the animal is alive, the pupil immediately dilates to the utmost, and remains immoveable, the iris being seemingly paralyzed."

We shall pass over the account of the voluntary movements of the body, to conclude our short notice of Mr. Mayo's work, with the following extract upon the subject of the reparation of broken or divided parts.

"When a considerable portion of skin is removed, the cellular membrane inflames below the blood, which stiffens on the raw surface, and secretes pus, and forms a crop of granulations; these gradually rise to the level of, and higher than the surrounding skin; the secreting surface appears to diminish daily, and becomes in patches, or, at its edges, converted into a tender whitish substance, which thickening becomes opake, and forms a cicatrix: on the day that a portion of a cicatrix is completed, it is insensible; about a fortnight afterwards it feels as if pricked with a needle.

"Thus in the formation of a cicatrix after destruction of the skin, the new material is produced, not by the neighbouring cutis, but by a growth from the subcutaneous texture.

"When tendons, or nerves, cartilages, or bones, are divided or broken across, the process of their reunion does not resemble the adhesion of divided skin, but has more in common with the growth of a cicatrix. The disjoined surfaces do not cohere immediately, but through the intervention of a third substance, which appears produced from the neighbouring cellular texture.

"If the tendo achillis be examined in a dog forty-eight hours after division, upon removing the skin, the subjacent cellular membrane, which surrounds the tendon, appears loaded with coagulable lymph and extravasated blood.

Upon making a longitudinal section of the thickened substance, the cut ends of the tendon contained within it are found to be about an inch apart, but connected together by means of coagulated blood, and swollen cellular texture.

"If the tendo achillis be examined seven days after division, the ends of the divided tendon are found united by an intervening substance of greater thickness than the tendon itself, that is readily separable from the skin and subjacent parts. Upon a longitudinal section being made, the intervening substance appears of a dark red colour, firm, and to a certain degree elastic: it coheres in some parts firmly, in others slightly, with the cut ends of the tendon, but strongly and inseparably with the cellular sheath of the tendon, which is discoloured for some distance: so that either end of the tendon admits, without much force, of being displaced from a socket in the intervening substance.

"At seventeen days after division, the intervening substance is found diminished in thickness, firmer, paler, and inseparably coherent with the cut ends of the tendon, the nature of which it gradually assumes.

"When a nerve is divided, the process by which the ends are joined exactly resembles the mode in which tendons unite. Without detailing the appearances on dissection at an earlier period, let me describe the state of the part at the time, when the return of its function first manifests itself.

"The infraorbital nerve was divided on one end upon the cheek of a cat, and a portion about a line in length was removed from it. The skin of the upper lip immediately lost sensation. The wound, however, readily cicatrized; and by the twentieth day sensation appeared entirely restored. Upon examining the part at this period, the nervous fibrils appeared to be united by a thick layer of tough grey semitransparent substance. On making a longitudinal section of this substance, and of the nervous fibrils which entered it, the extremities of the divided filaments appeared nearly two lines asunder, and firmly coherent with the intervening substance: here and there a whitish fibril seemed to extend further into the connecting medium, but no restoration of continuity between the nervous fibrils was observable.

"When the portio dura is divided on the cheek, it unites in a similar manner; but the nerve does not begin under four weeks to resume the office of transmitting the influence of the will. About this time, however, the eye-lids, which hitherto have been motionless, are observed to be slowly and imperfectly drawn towards each other whenever the surface of the conjunctiva is touched."

The remarks which follow show that the adjacent cellular texture is equally important in the reparation of cartilage and of bone; a conclusion, the justness of which is borne out by (at the same time that it seems to explain) the want of bony union in fractures of the neck of the femur within the capsule.

We have thus given a few extracts or the substance of a few passages in Mr. Mayo's Outlines of Physiology: but it is not possible to convey, by these means, a just notion of the merits of a systematic work; and we recommend those who are interested in medical science, to peruse for themselves the volume to which we have thus called their attention.

Report on the subject of Weights and Measures made to the Commissioners for Revising the Laws of the State of New York. By James Renwick, Professor of Natural Philosophy and Chemistry in Columbia College.

[Communicated by the Author, with the consent of the Commissioners to whom it is addressed.]

(1.) It is conceived that it would be inexpedient, indeed it may be said, impossible, to change the present denominations of weight and measure. The terms foot, yard, pound, ounce, bushel, gallon, quart, &c. are identified with our language; and it would be as easy to subvert the form and dialect of our ordinary colloquial speech, as to introduce new standards of weight and measure, with new names. All that could reasonably be attempted, is to refer the units of the several denominations to some measure in nature, constant, determinate and easily determinable, making such slight changes, not appreciable in the transactions of trade, as will tend to the facility of this determination; and, in addition, the means by which the accuracy of existing standards may be tested in future times, or the standards themselves restored, if lost, must be defined. To do more would be to attempt what is wholly beyond the reach of legislative power.

That this view of the subject is correct, may be shown by an appeal to the experience of the people of France. At the breaking out of the Revolution in that country, the steps necessary to form a system of weights and measures entirely different

from those formerly existing, and resting not merely for their standards, but for their absolute unity, upon a measure deduced from a constant dimension in nature, were undertaken. The result of this investigation was in a system that, if tested by the facility of applying to it the principles of decimal arithmetic, by the scientific and practical skill of the parties employed in the task, or by the great zeal and intelligence shown by them, is deserving of all praise. But in spite of the favourable circumstances existing at that era, when the whole nation was searching after novelties, and no prejudice in favour of any ancient customs could be considered as opposing, it was found that it could not be introduced in its full extent; and that the part on which the proposers most prided themselves, namely, the decimal division, by which it became accommodated to the existing scale of arithmetic magnitude, was the first which required modification. On the 12th of March, 1812, previous to the downfall of the Emperor Napoleon, the ancient denominations of lineal dimension, toise, foot, inch, &c. were restored, but appropriated to measures derived from the metrical system; and since the restoration of the Bourbons, that system has been further modified by the application of the ancient name of pound to the half kilogramme, with which that weight nearly corresponds. Nor does the necessity of making these changes arise from the inveteracy of prejudices, from which the French nation might be considered at the time as almost entirely free, but from causes actually existing in nature. The decimal notation, although long use has made it habitual, is by no means the most convenient for the calculations of arithmetic; it seems to have originated in no other cause than the habit of counting upon the fingers in the infancy of society; and although, from established habit, it would be proper in the ascending scale of weights and measures in ordinary use, and in the descending scale in scientific inquiry, is not applicable to the divisions of the unit in traffic. For this last purpose, a system admitting of binary division is alone fitted; and with this the duodecimal division of the foot is sufficiently agreeable; while it is found in the full extent in the customary divisions of the yard, the avoirdupoise pound, and the bushel. Warned, therefore, by the example of the French, we should attempt no change of names,

or new methods of division, for the several units of length, weight, and capacity; while, on the other hand, we learn that slight modifications of the magnitudes of either of these, that will render them more consistent with each other, and with a standard existing in nature, if such be attainable, are perfectly possible.

(2.) It is a well-known and established physical fact, that the pendulum vibrating seconds of mean time in any given place, is invariable in length. That is to say, that if made of a substance susceptible of variation in length with changes of temperature, the variation of its length will be attended with corresponding variations in the time of its oscillations; while, if so constructed as to remain invariable in length, the duration of its vibration in very small circular arcs, and in an atmosphere of unvarying pressure, will be constant: and although no experimental pendulum is invariable in length under differing temperatures, nor the pressure of the atmosphere constant, yet these two circumstances may be abstracted from, by means of corrections deduced from accurate experiments on the law of expansion by heat, and on the buoyancy of the atmosphere. The length of the seconds pendulum varies with the change of latitude, according to a regular and well-known law; and it has lately been shown to be affected by local circumstances. Still, however, the length of the seconds pendulum in a given position, when corrected for changes of temperature, and atmospheric pressure for the magnitude of its arc of vibration, and reduced to the level of the sea, is a standard, determinate and invariable in its dimension, that does exist in nature: it is also determinable, without any great difficulty, by persons furnished with the proper instruments, and possessed of the requisite scientific knowledge. For the best methods of determining the length of the pendulum, the world is indebted to Borda and Kater; and the method of the last is more especially remarkable as one of those brilliant discoveries that mark eras in science, and confer immortality on their inventors. The pendulum then should be recognised in the revised laws as the instrument whereby the standards may be restored, if lost; and with whose dimensions they are to be at present compared, by

way of making them comparable with the measures of foreign nations. In confirmation of this opinion, I may urge the examples of the French, the Danish, and the British governments, and that, still more recent, of Sweden.

(3.) As to the mode of determining the unit of length in relation to the standard existing in nature, and of deducing thence the standard of weight, and of measures of capacity.

I shall assume that the unit of lineal dimension that will be adopted by the State, is the yard in common use on the day of the declaration of independence, as prescribed in the law of 1784. This was, no doubt, identical with the British parliamentary yard made in 1760. It luckily so happens, that we possess the measure of the seconds pendulum made in the buildings of this institution in reference to that very standard, and consequently the means of making a yard that shall be identical with the yard of the Revolution. For this purpose it will be sufficient that the law should recognise that the standard yard shall bear a certain proportion to that measure of the pendulum. When the necessary experiments shall have been made to determine the true length of this yard, its extremities should be marked upon disks of an imperishable material, (gold or platinum, for instance,) inserted at a proper distance in a bar of brass, or rather of a more durable alloy of copper than what, in ordinary language, is called by that name. The distance between the two extremities of a bar is inadmissible in a standard, in consequence of the alteration in length that is caused by wear, as known in practice in the history of the yard preserved in the British Exchequer.

Such a bar will be subject to dilation and contraction by heat and cold; but the temperature at which its length is compared with the pendulum should be given, and when it is to be compared with, or its length transferred to, bars of the same material, as both will be equally affected, no legal enactment is in this case requisite; but as measures in ordinary use are most frequently of other materials, it will be necessary that the temperature at which such comparisons must be made, or to which they must be reduced by calculation, should be strictly specified. It would be proper to seek for this purpose a tempera-

ture, independent of the indications of instruments that, in the improvement of science, may become obsolete, and constant in all varieties of physical circumstances. Such a temperature is marked in nature while the process of liquefaction is going on, or what is commonly called the freezing point of water. This, therefore, should be declared, in the new law, to be the temperature at which such comparisons should be made, or to which they should be reduced.

The extension in length of the unit of lineal dimension will furnish the elements of measures of a larger size; its square, or that of one of its multiples, will be the unit of superficial measure, and its cube of solid.

To determine the unit of weight, it will be necessary to have recourse to some fluid that can at all times, and under all circumstances, be obtained pure and homogeneous. Distilled water is a fluid of this nature, and the law should declare that the weight of a certain bulk of it shall be equal to a certain defined proportion, or multiple of the unit of weight. To make this declaration specific, the circumstances under which the comparison is to be made, must be declared in the law; for water, like all other substances, will be of different weights under equal bulks, at different temperatures; and its apparent weight will be affected by the varying pressure of the atmosphere, as well as that of the weights employed, while their absolute gravity is that they would possess *in vacuo*. There appears at first sight to be a practical difficulty in this determination, in consequence of its being almost impossible to ascertain with the necessary precision the interior dimensions of a vessel; but we have a remedy in the well-known physical fact, that a solid body loses, when weighed in a liquid, a portion of its weight of an equal bulk of the liquid; and the exterior dimensions of a regular solid are determinable within the requisite limits. But this experiment will be affected by the calorific expansion of the substance employed, and by the buoyancy of the air affecting its absolute weight. The law then must not only express the absolute weight (in vacuo) of a certain bulk of water in terms of the standard of weight, but must describe the temperature of the water, and the substance of which the weights and the experimental solid are to be made. It so

happens, that in this case also we have a temperature, or rather a state of water, defined by a physical fact independent of instruments or extrinsic causes. Water does not follow the general rule of contraction, by diminution of heat at all temperatures, but, after contracting to a certain limit, again expands while cooling for several degrees above its freezing point. This fluid, therefore, has a maximum of density, and varies so slowly in bulk on each side of this maximum, as to afford room for the greatest accuracy of experiment.

Two methods exist of defining the magnitude of measures of capacity. The first and most obvious would be to prescribe the cubical dimensions of the unit in terms of the unit of length; but this is liable to the objection already stated, viz., the great difficulty of ascertaining with accuracy the internal dimensions of a vessel; the second consists in prescribing the weight of water that the measure shall hold, and this is in all respects preferable, but especially so from the very great ease with which the determination can be made. Here also it will be necessary to declare the atmospheric pressure and temperature at which the experiment shall be made, and this may be simply done by enacting that the water shall be at its maximum density, and pressure of the atmosphere at a mean.

(4.) The law of the province of New York of 1703, the act of 1784, and the revised law of 1813, recognise two standards of weight, without prescribing their relation to each other. Of these, the troy pound has long ceased to be used in any purely mercantile transaction, except in the retail trade of the apothecary; gold and silver are indeed also weighed by the troy pound: the former object is of very trifling importance, and the latter being governed by the practice of the mint of the United States, may be considered as a subject on which the national government has virtually legislated, and on which it would be improper for the state government to act. No other unit of weight then should be retained but the avoirdupoise pound; and as the quintal of 112lbs. has been abolished in practice, no other denomination of that species of weight, except the usual binary subdivisions of the pound.

Did the original law of 1703, or the act of 1784, authorise

two different units for liquid and dry measures of capacity, I should recommend the abolition of one of them as useless, and particularly from the confusion that arises from two different magnitudes being called by the same name. But it appears evident from the law of 1784, that but one measure is contemplated for both objects. The introduction of the British wine gallon is, therefore, unwarranted by the law of 1703, by the ancient practice of the province, or by the law of 1784, and has probably crept into the revised law of 1813, in consequence of its being used in the Customs of the United States: it is, therefore, consistent with the ancient law to prescribe but one standard for measures of capacity; but it might be expedient to permit the retail of articles paying duties to the United States in measures derived from the offices of the several surveyors of the Customs.

(5.) The act of 1784 adopts, as the standards of the state, certain weights and measures in the custody of William Hardenbrook, the sealer of the city and county of New York; and directs that he shall deliver them to the clerk of the city, in the presence of the mayor, recorder, and one or more aldermen, declaring upon oath that they are the identical articles received by him from the British exchequer. The importance of the object justified the solemnity of the manner pointed out for the transfer of these ancient and venerable standards of the province to the custody of the authorities of the state. But no corresponding means of preserving them were pointed out, and it is to be believed that they are no longer in existence; for a law of the 24th of March, 1809, adopts as the standard yard a brass measure, procured by the corporation of New York, in 1803, from the British exchequer; the liquid measures of capacity in the custody of the Secretary of State bear date in 1822, (at least nine years later than any existing law,) and the standards of dry measure are dated 1804. Now, as it has been found that the yard, purporting to be a copy of that described in the law of 1809, and deposited in the Street-commissioner's office, in this city, is shorter than the usual English yard of the beginning of the present century, which was itself shorter than the standard, as examined in 1760, and perpetuated by the cele-

brated *Bird*, and as we know of no adequate means employed in 1804 and 1822 to determine the truth of the new measures of capacity, I conceive I am warranted in stating that the original standards of weight and measure of this State, as they existed on the day of the declaration of its independence, have been lost; and the present is one of those junctures that call for scientific investigations to recover such of the ancient denominations as it may be expedient to restore identically, and to deduce from them the others in the most unexceptionable manner. With a view to this object, I have already stated that I consider the parliamentary standard of 1760, as unquestionably identical with the standard of our state as it existed at the Revolution, and that the experiments of Sabine, in Columbia College, have provided the facility, within our own state, of restoring the yard to its original magnitude. After the experiment shall have been made in this building, the measure should be transferred to some proper public edifice, the site at least of which may, in all succeeding ages, be resorted to, in order to confirm, or restore to their primitive dimension, existing weights and measures.

If, however, it should be preferred that the yard at present in the office of the Secretary of State should be the standard, it will be essential, in order to perpetuate it, to make similar experiments of comparison with the pendulum, one set of which should be made in this institution, as in this way alone its ratio to foreign measures (particularly to those of England) can be obtained. But I conceive this plan to be objectionable in several points of view, particularly as more difficult and expensive, and as adopting for a standard a measure whose authority is at least questionable. It would also prevent any positive legislation on the subject until after the experiments shall be made. The English avoirdupoise pound, as a separate standard, appears to have been lost. So long since as 1798, Sir George Shuckburgh could find none in the public offices that could be considered as authentic*, and it has, therefore, been customary in England to deduce it from the troy pound, on the principle that it was equal to 7000grs., of which the troy pound contained 5760. The experiments of Graham, in 1743, on weights of

* Philosophical Transactions Abridged, vol. xviii.

15lbs., make the avoirdupoise pound of which, at that time, authentic standards still remained, equal to 6998.5 grs. troy, while on the comparison of the single pound it was equal to 7004 grs.* The avoirdupoise pound might, therefore, be fairly taken at 7000 grs. troy. But this is evidently of the customary weight as usually employed, and not of the pound discovered by Kater, in the custody of the clerk of the British House of Commons. Of the customary weight used in the mints of this country and England, the weights of Sir George Shuckburgh made by the celebrated Troughton may be considered as the most authentic specimen. Assuming the true magnitude of the avoirdupoise pound to be 7000 grs. troy, the weight of a cubic foot of water, as calculated from the experiments of Shuckburgh, at its maximum of density, and weighed with brass weights, is 999 ounces and $\frac{67}{100}$, the measure of the experimental solid being performed upon a scale, whose standard temperature is 62° of Fahrenheit†. The experiments of Shuckburgh have been recently repeated by Kater‡, and from these I have calculated the weight of a cubic foot of water at its maximum of density to be 999 $\frac{88}{100}$ ounces, assuming the measure of the experimental solid to be made on a scale whose standard temperature is the melting point of ice: no danger then need be apprehended in taking the weight of a cubic foot of water at 1000 ounces avoirdupoise, and prescribing that the legal weight shall be such as will give that determination.

The discrepancy between this and the present received weights (if accurate transcripts of the former British standards) will not exceed three-tenths of an ounce in a hundred pounds, a quantity absolutely insensible in the course of traffic, less than the difference in the current standards of the avoirdupoise pound, in 1743, and far less than the amount of discrepancy discovered in the several States of the Union, as detailed in the able report of Mr. Adams.

For the unit of measures of capacity, I would propose the adoption of the gallon. It is sufficiently large to prevent any

* Philosophical Transactions Abridged, vol. xviii.

† Young's Natural Philosophy, vol. ii.

‡ Third Report to the British Parliament on weights and measures. Philosophical Transactions for 1821.

chance of error in the determination of the most usual denomination of dry measure (the bushel,) and it is itself the customary unit of liquid.

We shall now proceed to consider what ought to be the size of this unit. The Act of Parliament, of 13th William III. cap. 5. prescribes that the standard bushel shall contain 2150 inches. But among the standards preserved in the British Exchequer, for the bushel and its several parts, considerable discrepancies exist, and it would be difficult to determine from which of these the bushel of the Revolution was derived. Their several magnitudes are as follows:—

	Bushel.	*Gallon.*
	2215.2	276.9
	2234.4	279.3
	2163.2	274.4
	2128.9	266.1
Mean	2180.4	272.5

Still greater discrepancies exist in the usual bushels of different parts of the United States, which vary in the two extremes from 1925 to 2358.6 inches, (giving the gallon the two dimensions of 241.25 and 294.8,) as stated in the report of Mr. Adams.

Instead, however, of prescribing the cubic contents of measures of capacity, it would, as has been already explained, be much better to determine their magnitude by declaring the number of pounds of distilled water at the maximum density and mean pressure of the atmosphere that the unit should contain. If this weight be taken at 80 lbs., the bushel will be 2214.36 cubic inches, and the gallon 276.8. These dimensions differ less from the standard now adopted in England of 2217.6 and 277.2 than either of the above dimensions do from each other.

This method has a great advantage over that of actual measurement, inasmuch as the weight can be determined with sufficient accuracy, wherever good balances and just weights are to be had: while, to determine the cubic contents of a vessel is one of the most difficult operations of practical geometry, and is liable to such uncertainty, that in all recent investigations into the magnitude of measures, their weight when filled with

distilled water has been resorted to in preference as the surest criterion.

It might, in this stage of the discussion, be asked, Why propose a system founded upon principles and facts requiring so much accuracy of determination and knowledge of science? And, Why not have recourse to England at once, and obtain thence their new standards?

To the first of these it may be replied, that the practical adjustment of weights and measures is always liable to certain errors; that these, in the lapse of years, where no checks are provided, would reach such an extent as wholly to obliterate the recollection of the original standards. The more strict then the provisions of the law, in respect to accuracy, the less will be the danger of error, from the discouragement it will give to rude and inartificial methods of comparison; and the more scientific the means of recovery and examination that are pointed out, the greater will be the certainty of perpetuating or restoring the standard in after-ages. As to a recurrence to England, a variety of reasons would oppose, each of them valid and sufficient in itself: a proper feeling of national pride would forbid our asking aid of a foreign country, in a matter for which our own artists and men of science are competent. Economy also forbids it, as standards identical with those of the British government could only be obtained by sending out a competent person to superintend their construction, and compare them strictly with the originals. The articles vended in trade as accurate measures are not to be depended upon implicitly, and, however suitable to commerce, would be unfit for a national object; moreover, there are certain inaccuracies in the English law itself, which I cannot but believe must ere long require its revision in that country, and we should in that event obtain and bring into use a system differing from that which now exists among us, with every probability that it could not be permanent. Neither is it likely that any agent of our State, however fully authorised and accredited, could obtain all the necessary facilities.

The errors I allude to in the English law, which are fatal to its own permanence, are—that the comparison with the pendulum that is used to define the unit of lineal dimension

has been made in a private house, that cannot be reasonably considered as accessible for public purposes; that the length there determined has been assumed to be the universal length in the same parallel of latitude, while it has been conclusively proved that this length is influenced by local causes, a fact, that although not then absolutely demonstrated, was so far foreseen by the French commission, that they define, as the means of restoring the metrical system, if lost or impaired, not the pendulum of the latitude of Paris, but that of a specific place, the national observatory in that city; that the unit of weight (one nearly abandoned in this country) is derived from an experiment that requires a legal enactment to define and perpetuate an arbitrary thermometric scale, instead of taking, as is here proposed, a temperature defined by a physical fact, or law of nature, unsusceptible of modification or mistake under any circumstances; that the bulk of the water whose weight is given (a cubic inch) is much too small for accuracy; and that the unit of measures of capacity is deduced from that of weight, upon principles equally objectionable with those on which the latter is derived from the lineal measure. In addition, it is to be remarked, that the direct comparison and transfer of measures of length is hardly susceptible of accuracy equal to that obtained by means of the pendulum; that the system proposed, looking for its era to the date of the declaration of the independence of our State, would be exclusively and pre-eminently American. Another strong and important argument may be used: the city of New York possesses artists adequate to the construction of almost every part of the necessary apparatus; their skill and talent is now devoted to objects of inferior importance, that neither enhance their reputation nor add to the mechanical credit of the country: let them be employed upon an object of national, nay, universal, interest, and the name they will acquire will probably create a branch of commerce in similar articles, that will add much to the wealth and resources of the state. The importance of this argument was well understood by the French government, and such was success of the attempt, that it transferred to France, for the supply of its own demands, a branch of manufacture that had previously carried large sums to Great Britain, and struck the

first blow to the hitherto pre-eminent reputation of the articles of that country.

(6.) From what has already been stated, it will appear that I am not of opinion that the pendulum of lat. 45°, the northern boundary of our state, could be advantageously used, either as the unit of lineal measure, or as the standard existing in nature whence the unit of lineal measure is to be derived, and with it all other measures.

This, however, is a subject on which I venture to speak not without some hesitation; the authority of Mr. Jefferson is so high, both as a man of science and a statesman, that I do not differ from him without a feeling of extreme diffidence. Still, however, being satisfied that in the present state of knowledge, he would have concurred with me, and that the difference is one, not of opinion, but of existing facts, I do venture to differ from his views, stated as long ago as 1790.

I do not conceive that the pendulum of that latitude is to be preferred before that of any other as a standard existing in nature, while, as the absolute unit of length, it must be rejected upon the ground of the impossibility of introducing it, in consequence of its discrepancy, and that of all its derivatives from the habitual measures of the country. The latitude of 45° appearing at the first view as the mean between that of the Equator and the Pole, might been thought peculiarly advantageous. On this head it is to be observed, that it is a matter of question at the present moment between the governments of the United States and Great Britain, what this lat. of 45° is; are we to take for it the latitude determined by Astronomic observation at the surface of the earth, or the latitude corrected for ellipticity which is known by the appropriate name of Geocentric? And in connexion with the present subject, a third view of the question arises, for neither of these is distant from the Equator, the half of the itinerary distance between that circle and the Pole, but the point of bisection falls between them; which, then, of these three positions are we to choose for our experiments, particularly when the two most adapted, and between which the choice would most reasonably appear to fluctuate, are actually within the limits of possession of another

nation? It is, besides, to be remarked, as has been before stated, that it is now well known that the length of the seconds pendulum does not depend upon the latitude merely, but is affected by the geological and mineral structure of the place where the experiment is performed, and its vicinity*. Neither is the pendulum of 45°, even were the earth a sphere and the density of its surface uniform, the mean pendulum, for that would be found in the latitude of 35° 16′. For these reasons I cannot see any important advantage to be gained from naming the pendulum of 45° in the revision of the statutes relating to this subject.

For all immediate purposes, the inference drawn from the experiments of Sabine in this institution may be received as sufficient, and the liberal spirit of the trustees guarantees that the place of experiment will be accessible for the purpose of the first investigations, and the construction of the standards to be deposited in the proper public office. But it cannot be too strongly urged, that this measure should be transferred to a building, the property of the state, as early as possible, and that in it the instruments and documents connected with this investigation should be deposited. For this purpose, a location near the present seat of government, or perhaps one more central, in relation to the ultimate population of the state, should be chosen; and a preliminary law should provide for this important object.

(7.) The legislative provisions necessary to attain the objects of a well-regulated system of measures, and its recovery, in case of loss, would be most advantageously comprised in two different acts; one to become a permanent part of the revised laws—the other to be preliminary and temporary. The provisions of the first should be:—

1. That there shall be but one standard of measure of length and surface, one of weight, and one of measures of capacity, in this state.

2. That the unit of lineal measure shall be the yard, as used in this State at the date of the declaration of its independence, and that for its more precise definition, and in order to

* Sabine. Experiments on the Pendulum.

its recovery in case of loss, it is declared (until the measure of the pendulum shall be transfered to some appropriate public building), that the said yard has been found, by experiments made with a pendulum, with a brass rod, at Columbia College, New York, in the latitude of 40° 42′ 43″, to bear to the pendulum of that place vibrating seconds in vacuo, and at the level of the sea, at the temperature of melting ice, the proportion of one million (1,000,000) to one million, eighty-six thousand, one hundred, and fifty-eight (1,086,158).* That the yard shall be divided into three equal parts, called feet, and each foot into twelve equal parts, called inches, and shall be measured between two points, engraven upon golden disks, inserted into a brass rod.

This yard will be identical with that adopted in England, each being taken at its standard temperature, according to the principle of comparison pointed out by Wollaston, and used by Kater in his experiments on the measures of France. I am not fully satisfied that this principle is correct, being rather inclined to think that measures should be compared at a common temperature; but it is that which is now received in practice, and has, besides, certain conveniences as applied to this subject, in keeping the proposed system of weight and measures of capacity more near in magnitude to those received in England.

3. That the unit of weight shall be the pound of such magnitude, that the weight of a cubic foot of distilled water at its maximum density, weighed in vacuo with brass weights, shall be sixty-two and a half ($62\frac{1}{2}$) pounds, that the pound shall be divided into sixteen equal parts, called ounces, of which parts the cubic foot of distilled water, under the same circumstances, shall weigh one thousand (1000) ounces.

4. The unit of measures of capacity, whether dry or liquid, shall be the gallon, which shall be a vessel of such capacity as to contain at the mean pressure of the atmosphere at the level of the sea, ten pounds of distilled water at its maximum density; that all other measures of capacity shall be deduced from the gallon by continual multiplication or division

* The length of the said pendulum is 39.10168 inches.

by the number 2, being in the descending scale, half-gallons, quarts, pints, half-pints, and gills; and in the ascending scale, pecks, half-bushels, and bushels. And that, for greater precision, the last, or measure of eight gallons, shall contain at the mean pressure of the atmosphere at the level of the sea, eighty pounds of distilled water at its maximum of density.

On the Manufacture of Dies for Medals and Coinage. By W. T. Brande, F.R.S., Prof. Chem. R. I., &c.

[The subject of this discourse is not, I apprehend, of any general interest, but it has, as far as I know, been no where particularly described, if we except a brief notice respecting it annexed to Mr. Mushet's valuable paper on the art of coinage, printed in the supplement to the Encyclopædia Britannica. I therefore thought it worthy the attention of the members of the Royal Institution, and trust that the following outline will not be unacceptable to the readers of the Journal.]

The first circumstance that claims particular attention in the manufacture of dies is the selection of the best kind of steel for the purpose, and this must in some measure be left to the experience of the die-forger, who, if well skilled in his art, will be able to form a tolerably correct judgment of the fitness of the metal for the purpose, by the manner in which it works upon the anvil. It should be rather fine-grained than otherwise, and above all things perfectly even and uniform in its texture, and free from spots and patches finer or coarser than the general mass. But the very fine and uniform steel, with a silky fracture, which is so much esteemed for some of the purposes of cutlery, is unfit for our present purpose, from the extreme facility with which it acquires great hardness by pressure, and its liability to cracks and flaws. The very coarse grained, or highly crystalline, steel, is also equally objectionable; it acquires fissures under the die-press, and seldom admits of being equally and properly hardened. The object, therefore, is to select a steel of a medium quality as to fineness of texture, not too easily acted upon by dilute sulphuric acid, and exhibit-

ing an uniform texture, when its surface is washed over with a little aqua-fortis, by which its freedom from *pins* of iron, and other irregularities of composition, is sufficiently indicated.

The best kind of steel being thus selected, and properly forged* into the rough die, it is softened by very careful annealing, and in that state, having been smoothed externally, and brought to a table in the turning lathe, it is delivered to the engraver.

The process of annealing the die consists in heating it to a bright cherry red, and suffering it to cool *gradually*, which is best effected by bedding it in a crucible or iron pot of coarsely-powdered charcoal, that of animal substances being generally preferred. In this operation it is sometimes supposed that the die, or at least its superficial parts, becomes super-carbonized, or *highly-converted*, steel, as it is sometimes called; but experience does not justify such an opinion, and I believe the *composition* of the die is scarcely, certainly not materially, affected by the process, for it does not remain long enough in the fire for the purpose.

The engraver usually commences his labours by working out the device with small steel tools, *in intaglio;* he rarely begins in *relief* (though this is sometimes done); and having ultimately completed his design, and satisfied himself of its general effect and correctness, by impressions in clay, and dabs, or casts,† in soft metal, the die is ready for the important operation of *hardening*, which, from various causes, a few of which I shall enumerate, is a process of much risk and difficulty; for should any accident now occur, the labour of many months may be seriously injured, or even rendered quite useless.

The process of hardening soft steel is in itself very simple,

* The art of forging dies requires much practice and experience, not only as to the choice of steel, but as to the manual part of the operation. They should be forged at a high heat, and great care should be taken to give a perfect and dense texture to the upper part of the die.

† Type metal is usually employed for this purpose. The die is warmed, and placed in a convenient box, in which the melted metal may be splashed, or *dabbed*, upon it. The impression is often extremely clear and perfect, and exceeds in effect those which are taken in wax or plaister.

though not very easily explained upon mechanical or chemical principles. We know by experience, that it is a property of this highly valuable substance, to become excessively hard, if heated and suddenly cooled; if, therefore, we heat a bar of soft malleable and ductile steel red hot, and then suddenly quench it in a large quantity of cold water, it not only becomes hard, but fragile and brittle. But as a die is a mass of steel of considerable dimensions, this hardening is an operation attended by many and peculiar difficulties, more especially as we have at the same time to attend to the careful preservation of the engraving. This is effected by covering the engraved face of the die with a *protecting paste*, composed of fixed oil of any kind, thickened with powdered charcoal: some persons add pipe-clay, others use a pulp of garlic, but pure lamp-black and linseed oil answer the purpose perfectly. This is thinly spread upon the work of the die, which, if requisite, may be further defended by an iron ring; the die is then placed with its face downwards in a crucible, and completely surrounded by powdered charcoal. It is heated to a proper temperature, that is, about cherry red, and in that state is taken out with proper tongs, and plunged into a cistern of cold water, of such dimensions as not to become materially increased in temperature; here it is rapidly moved about, until all noise cease, and then left in the water till quite cool. In this process it should produce a bubbling and hissing noise; if it pipes and sings, we may generally apprehend a crack or fissure.

I have found no process answer better than the above simple and common mode of hardening dies, though I have given others repeated and fair trials. It has been proposed to keep up currents and eddies of cold water in the hardening cistern, by means of delivery-pipes, coming from a height; and to subject the hot die, with its face uppermost, to a sudden and copious current of water, let upon it from a large pipe, supplied from a high cistern; but these means have not in any way proved more successful, either in saving the die, or in giving it any good qualities. It will be recollected, from the form of the die, that it is necessarily only, as it were, *case-hardened*, the hardest strata being outside, and the softer ones within, which envelope a core, something in the manner of the successive

coats of an onion; an arrangement which we sometimes have an opportunity of seeing displayed in dies which have been smashed by a violent blow.

The hardening having been effected, and the die being for the time safe, some further steps may be taken for its protection; one of these consists in a very mild kind of tempering, consisting in putting it into water gradually raised to the boiling point, till heated throughout, and then suffering it gradually to cool. This operation renders the die less apt to crack in very cold weather. A great safeguard is also obtained by thrusting the cold die into a red-hot iron ring, which just fits it in that state, and which, by contracting as it cools, keeps its parts together under considerable pressure, preventing the spreading of external cracks and fissures, and often enabling us to employ a split die for obtaining punches, which would break to pieces without the protecting ring.

If the die has been successfully hardened, and the protecting paste has done its duty, by preserving the face from all injury and oxydizement, or *burning*, as it is usually called, it is now to be cleaned and polished, and in this state constitutes what is technically termed a MATRIX: it may of course be used as a source of medals, coins, or impressions, but it is not usually thus employed, for fear of accidents happening to it in the coining press, and because the artist has seldom perfected his work upon it in this state. It is, therefore, resorted to for the purpose of furnishing a PUNCH, or a steel impression in *relief*. For this purpose a proper block of steel is selected, of the same quality, and with the same precautions as before, and being carefully annealed, or softened, is turned like the matrix, perfectly true and flat at the bottom, and obtusely conical at top. In this state, its conical surface is carefully compressed by powerful and proper machinery upon the matrix, which being very hard, soon allows it to receive the commencement of an impression; but in thus receiving the impression, it becomes itself so hard by condensation of texture, as to require during the operation to be repeatedly annealed, or softened, otherwise it would split into small superficial fissures, or would injure the matrix. Much practical skill is therefore required in taking this impression, and the punch, at each

annealing, must be carefully protected, so that the work may not be injured.

Thus, after repeated blows in the die-press, and frequent annealing, the impression from the matrix is at length perfected, or completely *up*, and having been touched up by the engraver, is turned, hardened, and collared, as the matrix, of which it is now a complete impression in *relief*, and, as we have before said, is called a *punch*.

This punch becomes an inexhaustible source of *dies*, without further reference to the original matrix; for now by impressing upon it plugs of soft steel, and by pursuing with them an exactly similar operation to that by which the punch itself was obtained, we procure impressions from it to any amount, which of course are *fac-similes* of the matrix, and these dies being turned, hardened, polished, and, if necessary, tempered, are employed for the purposes of coinage.

The distinction between striking medals, and common coin, is very essential, and the work upon the dies accordingly adjusted to each. Medals are usually in very high relief, and the effect is produced by a succession of blows; and as the metal in which they are struck, be it gold, silver, or copper, acquires considerable hardness at each stroke of the press, they are repeatedly annealed during the progress of bringing them up. In a beautiful medal, which, as well as the dies for it, I have had lent me by Mr. Wyon, and which he has just completed for the Royal Naval College, the obverse represents a head of the King, in very bold relief; it required thirty blows of a very powerful press to complete the impression, and it was necessary to anneal each medal after every third blow, so that they went ten times into the fire for that purpose. In striking a coin or medal, the lateral spread of the metal, which otherwise would ooze out as it were from between the dies, is prevented by the application of a steel collar, accurately turned to the dimensions of the dies, and which, when left plain, gives to the edge of the piece a finished and polished appearance; it is sometimes grooved, or milled, or otherwise ornamented, and occasionally *lettered*, in which case it is made in three separate and moveable pieces, confined by a ring, into which they are most accurately fitted, and so adjusted that the metal may be forced into the

letters by its lateral spread, at the same time that the coin receives the blow of the screw-press.

Coins are generally completed by one blow of the coining-press. These presses are worked in the Royal Mint by machinery, so contrived that they shall strike, upon an average, sixty blows in a minute; the blank piece, previously properly prepared and annealed, being placed between the dies by part of the same mechanism. Of these presses, and of the other machinery and operations used and carried on in the Mint, an excellent account, with illustrative engravings, has been given by Mr. Mushet, in the supplement to the Encyclopædia Britannica.

The number of pieces which may be struck by a single die of good steel, properly hardened and duly tempered, not unfrequently amounts at the Mint to between three and four hundred thousand, but the average consumption of dies is of course much greater, owing to the different qualities of steel, and to the casualties to which the dies are liable:—thus, the upper and lower die are often violently struck together, owing to a fault in the *layer-on*, or that part of the machinery which ought to put the blank into its place, but which now and then fails so to do. This accident very commonly arises from the boy who superintends the press neglecting to feed the hopper of the layer-on with blank pieces. If a die is too hard, it is apt to break or split, and especially subject to fissures, which run from letter to letter upon the edge. If too soft, it swells, and the collar will not rise and fall upon it, or it *sinks* in the centre, and the work becomes distorted and faulty. He, therefore, who supplies the dies for an extensive coinage has many casualties and difficulties to encounter. There are eight presses at the Mint, frequently at work for ten hours each day, and I consider that the destruction of eight pair of dies per day (one pair for each press), is a fair average result, though we much more frequently fall short of than exceed this proportion. It must be remembered, that each press produces 3600 pieces per hour, but, making allowance for occasional stoppages, we may reckon the daily produce of each press at 30,000 pieces; the eight presses therefore will furnish a diurnal average of 240,000 pieces.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

i. *Table of the* Depression *of* Mercury *in the* Tube *of a Barometer, including that of* Mr. Bouvard *(*Conn. des Tems, 1829*).*

Diameter.		Central Depression.				
Inches.	Millimetres.	Bouvard, 1826.		Mr. Ivory, 1820.	Dr. Young, 1818.	Observed, Ld. C. C.
1.0000	25.401				.00031*i*	
.8268	21.000	.028*mm*	.00110*i*			
.8071	20.500	.032	.00126			
.8000	20.321		[.00132]	.00119*i*	.00115	
.7874	20.000	.036	.00142			
.7677	19.500	.041	.00161			
.7480	19.000	.047	.00185			
.7284	18.500	.053	.00209			
.7087	18.000	.060	.00236			
.7000	17.780		[.00250]	.00228	(.00220)	
.6890	17.500	.068	.00267			
.6693	17.000	.077	.00303			
.6496	16.500	.087	.00343			
.6299	16.000	.099	.00390			
.6102	15.500	.112	.00441			
.6000	15.240		[.00472]	[.00431]	.00416	.005*i*
.5906	15.000	.127	.00500			
.5709	14.500	.143	.00563			
.5512	14.000	.161	.00634			
.5315	13.500	.181	.00713			
.5118	13.000	.204	.00803			
.5000	12.701		[.00863]	.00835	(.00799)	.007
.4921	12.500	.230	.00906			
.4725	12.000	.260	.01024			
.4528	11.500	.293	.01154			
.4500	11.430		[.01175]	.01117	(.01099)	
.4331	11.000	.330	.01299			
.4134	10.500	.372	.01465			
.4000	10.160		[.01590]	.01534	.01503	.015
.3937	10.000	.419	.01650			
.3740	9.500	.473	.01862			
.3543	9.000	.534	.02103			

DIAMETER.		CENTRAL DEPRESSION.				
Inches.	Millimetres.	Bouvard, 1826.		Mr. Ivory, 1820.	Dr. Young, 1818.	Observed, Ld. C. C.
.3500	8.890		[.02163]	.02110	(.02082)	.025*i*
.3346	8.500	.604*mm*	.02378			
.3150	8.000	.684	.02693			
.3000	7.620		[.02965]	.02916	(.02881)	.036
.2953	7.500	.775	.03051			
.2756	7.000	.877	.03453			
.2259	6.500	.995	.03917			
.2500	6.350		[.04084]	.04075	(.04025)	.050
.2362	6.000	1.136	.04473			
.2165	5.500	1.306	.05142			
.2000	5.080		[.05806]	.05811	.05798	.067
.1969	5.000	1.507	.05933			
.1772	4.500	1.752	.06898			
.1575	4.000	2.053	.08083			
.1500	3.810		[.08350]	.08628	(.08568)	.092
.1378	3.500	2.415	.09508			
.1181	3.000	2.902	.11425			
.1000	2.540		[.13910]	.14028	.14004	.140
.0984	2.500	3.594	.14149			
.0787	2.000	4.579	.18028			
.0500	1.270			.29494	(.29502)	

The numbers inclosed within parentheses are derived from an empirical interpolation.

If we compare this table with that which was published in the Connaissance des Tems for 1812, and thence in the Philosophical Magazine for February 1816, we shall be disposed to admire the *perseverance* of Mr. Laplace and Mr. Bouvard, in still adhering to the very inartificial mode of calculation which was first employed by Dr. Young in this country, but rejected by him as soon as he had discovered a more correct, though an equally laborious method of solving the problem.

The calculations of Dr. Young and of Mr. Ivory, as published in the Supplement of the Encylopædia, articles Cohesion, and Fluids, are completely independent of each other, and their errors appear in general to be of an opposite character; the greatest difference of the results, for the depression, is .00031, or less than the three-thousandth of an inch, while the approximation of Mr. Bouvard differs from both

of them in one case as much as .00218, and in almost every case much more than they differ from each other.

After all, however, it is only in one or two of the cases computed, that any of the differences amount to one thousandth of an inch, and either of the tables may therefore be considered as sufficiently accurate for the practical correction of the height of the barometer. The error of Lord Charles Cavendish's experiments appears, in one or two cases, to have been ten times as great as this, which may have happened without any particular negligence in the observer.

ii. *Remarks on the* CHANGE *of Inclination of the* DIPPING NEEDLE. *By* Mr. ARAGO. *Annuaire*, 1827. p. 207.

At Paris the *declination* of the compass disappeared altogether in 1666, and, becoming westerly instead of easterly, continued to increase until 1819, when it was $22\frac{1}{2}°$, and has ever since that time been slowly diminishing.

The *dip* has continually diminished, from the date of the most ancient observations to the present time: it is now about 68°.

When the horizontal needle was found to be stationary, it "*was natural to suppose*," that the dipping needle would become stationary also, and that its inclination would afterwards be increased. But this conjecture has not been confirmed by observation; and the dipping needle, on the contrary, continues, as in times past, to approach nearer to the horizontal position. This is a circumstance which must be explained by those who attempt to investigate the cause, hitherto entirely unknown, of these singular changes.

[With all due deference to the opinion of so cautious and accurate a natural philosopher as Mr. Arago, the *law*, if not the *cause*, of these singular changes, may very easily be accommodated to the facts in question. For the simplest possible theory of the change of magnetism, is to suppose that the magnetic pole performs a certain orbit round the pole of the earth; and whether regular or irregular in its form, this orbit must, at some one point on each side, have for its tangent a great circle passing through the point of

observation. At the moment when the magnetic pole passes through this point, the declination or variation will obviously be a maximum, while the change of inclination will as obviously be greater than in any other part of the orbit, other things being supposed equal: since the pole will be moving directly to or from the place of observation. On the other hand, the change of declination will probably be the most rapid when the dip is the greatest, as it probably was in London and Paris, towards the middle of the 17th century, or a little later, the declination itself being inconsiderable, or somewhat westerly, because the nearest point of the orbit seems to be on that side of the true meridian of London.

It is singular that this useful little almanac should annually present to the public, among the "Epochs of the principal discoveries in Geography," that of the Canary Islands, by the Genoese and Catalonian navigators in 1345; while there seems to be no reason for doubting that these islands were well known to the ancients, one of them being described by Pliny under the name of Canaria. (vi. 32.)]

iii. *Experiments on the effect of the direct* WHITE LIGHT *of the Sun in making Steel* MAGNETIC. *By* Mr. A. BAUMGÄRTNER, *of Vienna.* Ann. Chim. Nov. 1826.

When I was repeating, in the course of last summer, the experiments of Mrs. Somerville, on the effect of the sun's coloured light in making steel magnetic, I discovered a process which appeared to succeed more rapidly and more certainly than that which Mr. Morichini and Mrs. Somerville employed. It led me to this conclusion, that a piece of steel, of the size of a common knitting needle, of which one or several parts are polished, and the remainder unpolished, when exposed to the direct white light of the sun, acquires the properties of a north pole at each polished part, and a south pole at each unpolished part. The process that I have employed is the following:

I took a piece of English steel, of the size of a common knitting needle, and heated it till it became covered with black oxyd; I then removed the oxyd at one or more parts, by rubbing it on a hone with oil, and finished the polishing

with chalk and a piece of lime-tree wood, so as to form on it shining zones, about two or three lines in length. The steel, thus prepared, being exposed in a place strongly enlightened by the sun, was found, after some time, to become strongly magnetic in the way that has been described. The time, other things being equal, seemed to depend on the intensity of the solar light; for when I concentrated the light, by means of a lens, on the polished parts, I obtained as strong a magnetism in a few minutes, as was produced by an exposure of several hours to the light in its natural state.

A piece of steel, merely polished at one of its extremities, receives a north pole at that end, and a south pole at the other. If the polished part is in the middle, the two extremities become south poles, and the middle a north pole. If the wire, on the contrary, is polished at the two ends, they become north poles, while the middle is a south pole. Lastly, if there are several polished zones, each of them receives the boreal magnetism, and the dark zones which separate them, the austral. In this manner we may obtain an unlimited number of magnetic poles, provided that the steel wire be of a length proportional to it. I have thus easily procured eight poles, in a wire eight inches long, although of unequal force: for I have constantly found that the poles at the ends were stronger than the rest, and that they preserved their magnetism for a longer time. I have not been able to magnetize by the same means wires, either entirely covered with oxyd, or perfectly polished, nor other wires with longitudinal polished stripes.

The whole of these results were the same, whatever might be the position of the wire with respect to the meridian; and each experiment was repeated several times without any variation in its success. It is unnecessary to add, that before each wire was exposed to the sun's rays, I carefully examined whether or no it was magnetic, and those wires which possessed any magnetic properties were rejected as unfit for the experiment.

iv. *Elementary view of the* UNDULATORY *Theory of* LIGHT. *By* Mr. FRESNEL. From the Supplement to the Translation of Thomson's Chemistry. 8. *Par.* 1822.

[The Council of the Royal Society having adjudged, on the 8th of February, Count Rumford's PRIZE MEDAL to Mr. FRESNEL, for his application of the undulatory theory of light to the phenomena of polarisation, and this decision having probably tended to give new weight to that theory in the estimation of the British public; it is apprehended, that a translation of Mr. FRESNEL's very distinct and well-arranged statement, of the grounds of that theory, will not be judged an improper article for insertion in the Nautical and Astronomical Collections.]

I. NATURE OF LIGHT.

The Nature of Light has long been a subject of dispute among natural philosophers: some of them suppose that it is a material substance, darted out by luminous bodies; and others, that it consists in the vibrations of an infinitely subtile elastic fluid diffused through all space, in the same way that sound is known to be a vibration of the air. The system of undulations, which was contrived by the genius of DESCARTES, and which was more ably completed in its detail by HUYGENS, has also been adopted by EULER, and in more modern times by Dr. THOMAS YOUNG, to whom the science of optics is indebted for many important discoveries. The system of emission, or that of NEWTON, supported by the great name of its author, and I might almost say, by that reputation for infallibility which his immortal PRINCIPIA had earned for him, has consequently been more generally adopted. The other hypothesis was apparently altogether abandoned, when Dr. YOUNG recalled the attention of philosophers to it by some curious experiments, which afford a striking confirmation of it, and which appear at the same time to be scarcely reconcilable with the system of emission.

A variety of new phenomena, observed since that time, have continued to add to the probability of the truth of the theory of undulations. Though long neglected, and more difficult to be followed, in the detail of its applica-

tions, than the hypothesis of emission, it has already enabled us to adapt our calculations much more correctly to the phenomena; and this is one of the least equivocal proofs of the truth of any system. When a hypothesis is true, it must lead us to numerical relations between facts the most different in their appearances: while a false hypothesis, on the contrary, may represent very accurately the class of phenomena from which it has been deduced, in the same way that an empirical formula may represent a limited number of results that have afforded it; but it can never trace the secret relations which connect these phenomena with others of a totally different class.

It was in this manner, for example, that Mr. Biot investigated, with as much sagacity as perseverance, the laws of the beautiful phenomena of colours which Mr. Arago had discovered in crystallized plates, and found that the tints exhibited by them followed, with regard to their thicknesses, laws similar to those of the coloured rings already known; that is to say, that the thicknesses of two crystallized plates of the same nature, which exhibited any given tints, were in the same proportion as two plates of air, which afforded similar tints in the production of coloured rings. This relation, indicated by analogy, without any regard to a particular theory, was without doubt very remarkable and very important; but Dr. Young advanced much further, by means of the law of interferences, which is an immediate consequence of the system of undulations. He discovered a much more intimate relation between these two classes of phenomena; that is, that the difference of the lengths of the paths of the rays ordinarily and extraordinarily refracted, in the crystallized plate, is precisely equal to the difference of the paths described by the rays reflected at the first and second surface of the plate of air that exhibits the same tint as the crystal: and the phenomena, instead of a simple analogy, are reduced to the predicament of identity.

I might add that the laws, so complicated in appearance, of the phenomena of diffraction, which had escaped all attempts to detect them with the assistance of experiments, as combined with the theory of emission, are perfectly con-

sistent, in all their extent, with the simplest principles of the theory of undulations. Without doubt observation has assisted also in the discovery of this relation; but observation alone could never have developed them; while in this case, as well as in many others, the theory of undulations might easily have gone before the experiments, and have predicted beforehand the precise results in their minutest details.

The example, which has already been mentioned, sufficiently proves that the choice of a theory is not a matter of indifference in the investigation of physical phenomena. Its utility is not confined to the purpose of facilitating the study of facts, by uniting them into groups more or less numerous, according to the relation which they bear to each other. Another no less important end of a good theory must be to contribute to the essential advancement of science, by the discovery of facts, and of relations between classes of phenomena the most distinct, and in appearance the most independent of each other. Now it is obvious that if we set out with an imaginary hypothesis respecting the cause of light, we shall not reach this end so readily, as if we possessed the true secret of nature. A theory depending upon a true fundamental hypothesis, however ill it may be suited for the application of mathematical analysis to the mechanical operations which it involves, will still lead us to intimate relations between distant facts, which would for ever have remained unknown upon any other suppositions. Hence, to say nothing of our natural curiosity to know the truth in all cases, we see how important it is to the progress of optics, and of the kindred science of chemistry, to know whether luminous molecules are actually projected from the sources of light and enter our eyes, or if light is propagated by the undulations of a continuous medium, to the particles of which, the luminous bodies communicate their vibratory motions. And we have no reason to believe that the decision of this question is impossible, because it has long remained unaccomplished: we may even venture to assert that, as far as probability goes, it is already determined; and that, after comparing attentively the application of the

two systems to all the facts which have been hitherto discovered, an impartial judge will not hesitate to admit the superiority of the theory of undulations.

In entering into the detail of the facts in question, it will not be advisable to separate them from the theoretical views which have led so immediately to their discovery; and it appears to be equally conducive to the purposes of instruction, and to the advancement of the science, to explain the essential principles and foundations of a theory which has been too long neglected and misunderstood: it will, however, be proper on this occasion to omit many details of calculation, for the sake of conciseness, and having reduced each case to the condition of a mathematical problem, it will be sufficient to take its solution for granted.

The diffraction of light will be considered in the first place, because it relates to the simplest possible case of a shadow cast by an opaque body illuminated by a single radiant point; and this case will be considered somewhat diffusely, as affording the best test of the comparative value of the two theories.

II. Diffraction of Light.

The term *Diffraction* is applied to those modifications which light undergoes in passing near the extremities of bodies.

When we admit the rays of light by a very small aperture into a dark room, we observe that the shadows of bodies, instead of being terminated abruptly and distinctly as they ought to be, if the light always passed by them in right lines, are softened in their outlines, and bounded by three very distinct fringes of colours, of unequal breadths, the first being the widest, and the third the narrowest; and when the body intercepting the light is narrow, we see fringes even in its shadow, which then appears to be divided by darker and lighter stripes, placed at equal distances from each other. We may call this latter species *internal fringes*, and the former *external*.

Grimaldi is the first philosopher that observed and studied these fringes with care. Newton, who investigated the subject of diffraction, and even devoted to it the last

book of his Optics, does not appear to have remarked the internal fringes, although his researches were subsequent to those of Grimaldi; for he says in the 28th query of his third book, where he states as an objection to the theory of undulation, that the luminous waves ought to spread into the shadow: "the rays which pass very near to the edges of any body are bent a little by the action of the body, as we showed above; *but this bending is not towards but from the shadow*, and is performed only in the passage of the ray by the body, and at a very small distance from it: so soon as the ray is past the body, it goes right on." It is difficult to understand how the inflection of light into the shadow should have escaped so accurate an observer, especially when we consider that his experiments were made on very narrow bodies, and sometimes even on hairs. We are almost compelled to suspect, that his theoretical prejudices had, in a certain degree, tended to shut his eyes to these important phenomena, which contribute very much to do away the objection, on which he so much depended for the superiority of his system.

Since this inflection of the light into the shadow is a leading fact in the theory, it becomes necessary to insist on the details of the experiment by which it is established. In order to make it in the most unexceptionable manner, let the sun's light be admitted into a dark room through a hole in the window-shutter, covered with a piece of tin foil, in which a hole is made with a pin, not exceeding $\frac{1}{250}$ of an inch in diameter; instead of allowing the sun to shine directly on this hole, let the light be first reflected by a small mirror into a direction nearly horizontal. Place now in the luminous cone, formed by the rays thus introduced, a perfectly opaque wire, about $\frac{1}{20}$ of an inch in diameter: suppose the distance of the iron from the hole to be about a metre or about a yard, and let the shadow be received on a screen of white paper at the distance of two metres or of two yards. If the small hole were of evanescent dimensions, it is plain that the geometrical shadow as projected on the card would be $\frac{2}{20}$ of an inch in diameter, that is, supposing the rays to suffer no inflection.

Let us now calculate how much the breadth of the absolute geometrical shadow must be reduced, on account of the dimensions of the hole: since we have supposed its diameter $\frac{1}{250}$ of an inch, the extreme rays will be at the distance of $\frac{1}{500}$ of an inch from the central rays, and the distance of the card from the wire being twice as great as that of the hole, the breadth of the penumbra will be $\frac{1}{250}$, and the true shadow instead of $\frac{3}{200}$ will be $\frac{6}{200} - \frac{2}{250}$, or $.15 - .008 = .142$; and this space ought to be perfectly dark if the rays underwent no inflection. Now if we observe the shadow attentively, we shall discover in it stripes slightly illuminated, separating some dark lines, the middle line being always bright: the term *bright* being always applied to a stripe lying between two darker stripes, whatever the actual intensity of its light may be. This experiment, so easily made, proves therefore that light is actually inflected into the shadows of bodies, as Grimaldi had remarked. It is indeed very rapidly weakened as the angle of inflection is increased; but this rapid decrease has nothing in it unfavourable to the theory of undulations, which explains it easily from the minute breadth of the separate undulations, and even assigns the law of its gradation. It is, therefore, obvious, that Newton was mistaken in denying the diffusion of light into the dark space behind an illuminated object, and the objection to the theory of undulations, which he derived from this observation, was founded on error.

In speaking of these internal fringes, it becomes necessary to describe the ingenious experiment made on these fringes by Dr. Young, and the important inference which he drew from them.

Having intercepted, by the interposition of a screen, all the light which passed on one side of the illuminated body, he found that the fringes situated within the shadow completely disappeared without leaving any traces of their existence, though only one half of the light concerned in the experiment was withdrawn. Hence he inferred, that the meeting of two beams of light was required for their production, and they were the result of the influences exerted by one of the two beams on the other: for since each of

them, taken separately, produced a uniform illumination only, their joint effect would also be a uniform illumination, unless they exerted some particular influence on each other.

If we supposed, as it is natural to do, in the system of emission, that the different inflections of the luminous rays, near the bodies, depend on a certain attractive or repulsive action, exerted by them on the particles of light, it might be imagined that, in this experiment, the action of the remoter edge of the wire might be modified by that of the screen which touched it, so as to be deprived of the property of producing the fringes. This objection might be very much weakened by observing, that the external fringes are not in the slightest degree affected by the neighbourhood of the screen; but Dr. YOUNG removed it altogether, by placing the screen at such a distance, that it could not be supposed to have any effect of this kind, and intercepting the beam of light either before or after its arrival at the edge of the wire; for in either case the internal fringes were completely annihilated.

He also showed the mutual influence of two pencils of light, by admitting them through two small holes near each other, and receiving them on a card at some distance from the window: the intermediate space was then occupied by dark and bright lines, which were obviously occasioned by the concourse of the two pencils, since they disappeared as soon as either of the pencils was intercepted, by stopping the hole that transmitted it.

The fringes are more distinct when, instead of making two holes in the plate of tinfoil, we make in it two parallel slits at the distance of $\frac{1}{10}$ or $\frac{1}{20}$ of an inch; we then destroy the fringes by stopping either of the slits, although the light spreading from the other remains still perfectly sensible. It often happens, when the slits are not very narrow, and when the light is received on a card pretty near to the window, that *some* fringes are still perceptible when one of the slits is stopped: but they are not the fringes in question; being easily distinguishable from them while the slits remain much narrower than the interval that separates them; for in

this case the internal fringes, which disappear, upon the interception of either of the pencils, are much finer than the others, which depend on each slit separately; and it is towards the middle of the space in which these coarser stripes mix with each other, that the finer lines are generated.

It is always supposed that the whole of the light, employed in these experiments, is derived from the same luminous point; for if it were otherwise, if the two pencils did not emanate from the same source, the effects of which we have spoken would no longer take place. The reason of this will appear hereafter: for the present, it will be best to confine ourselves to those facts which demonstrate, with the strongest evidence, that, in certain cases, the rays of light exert a sensible influence on each other.

In order to complete what belongs to this part of the subject, it is necessary to describe another experiment which exhibits this influence in a very neat manner, and has the advantage of separating it from the phenomena of diffraction properly so called. It consists in employing two mirrors, very slightly inclined to each other, for the reflection of rays proceeding from the same luminous point; and it will be best to premise an account of some more general improvements which may be made in the means of observation.

Instead of employing for a luminous point a small hole made in a piece of tinfoil, or of card placed in a window-shutter, it is much more convenient to insert in it a convex glass lens of a very short focus, on which the sun's rays are thrown horizontally by a mirror outside of the window. It is well known that the effect of such a lens is to unite into a point, called its focus, all such rays as fall parallel to each other on its surface; and that this focus is situated in the ray which passes through the centre of the lens, and is so much the nearer to it as the lens is more convex. In order to have a more distinct idea of the experiment, we may suppose this focal distance to be a centimetre, or about $\frac{4}{10}$ of an inch. If the sun, like the fixed stars, exhibited to us no appreciable angular diameter, all the rays after refraction would be united into a single focus; but the sun actually subtends an

angle of about 32′: that is, the rays coming from the opposite extremities of any of its diameters make with each other an angle of 32′. Now, in order to determine the magnitude of the image, we have only to follow these rays through the centre of the lens, till they arrive at the distance of the focus, that is, $\frac{4}{10}$ of an inch, and the chord of a little arc of 32′ in extent, of this radius, will be about $\frac{36}{10000}$ of an inch, that is, $\frac{1}{278}$: and such will be the diameter of the little image of the sun formed in the focus of the lens, by the rays thrown by the mirror upon its surface, which, after crossing in this point, will diverge into a luminous cone much more extensive than that which is formed by the sun's rays simply admitted into the hole, especially if the lens is of tolerable magnitude. This extension of the cone gives a great facility to the experiment; it was first pointed out to me by Mr. Arago, and I have since always used it in my experiments. It is also necessary to take the precaution of excluding the direct rays of the sun from the surface of the lens, in order to avoid the confusion that might arise from having a second luminous point producing a distinct series of phenomena.

If it is desired to have the luminous point absolutely fixed, as in the case of the accurate measurement of the relative situation of the fringes, it will be proper to employ, instead of a simple mirror, a heliostate, an instrument so called, from keeping the reflected ray in a constant direction, notwithstanding the diurnal motion of the sun. It is easy to perceive, that without this precaution the reflected rays, changing their direction with that of the incident rays, would cause a small change of place in the image which is formed by their convergence. But this perfect immobility of the luminous point is only necessary in the case of our wishing to measure the exact dimensions of the fringes: and even for this purpose we might dispense with it, by taking only a few measures at once; so that each operation might be performed in a short time, and by employing a lens of a short focus only.

Having described the best manner of forming a luminous point, I shall explain the most convenient mode of measuring

the fringes, following the steps by which it occurred to me.

Wishing to observe the external fringes very near the opaque body, I attempted to receive the shadow on a plate of ground glass, and to look at them from behind with a lens: and in moving my eye till it had passed beyond the glass, I remarked that they remained equally visible, and even more distinctly, and that they were precisely similar to those which appeared on the glass. Hence I inferred that the glass was altogether superfluous, and that it was sufficient to receive the light directly on the lens, looking at the luminous point from behind. The reason of this is very obvious: the effect of a convex lens is to give a clear picture to the eye of whatever is its focus, whether it may be a real object or an image formed by any arrangement of rays of light; provided that these rays fall on the surface of the lens without alteration. It is thus that the eye-glass of a telescope enables us to see the aërial image of the objects depicted in the focus of the object-glass; an image which is seen in the same form, though much less distinctly, when it is received on white paper or on ground glass. This mode of observation, which might have been suggested by theory only, is greatly preferable to that which had been commonly followed, because it has the advantage of magnifying the fringes, and of increasing their brilliancy at the same time; which permits us to distinguish them in a multitude of cases, when they could not be distinguished on a piece of a white paper, on account of their minuteness, or the weakness of the light. In order to see the fringes very distinctly, it is necessary to take care that the focus of the rays converging from the lens should fall on the middle of the pupil, by placing the eye at such a distance from it, that all its surface may appear illuminated when it is wholly exposed to the light, and keeping the distance unchanged when the lens is moved towards the shadow. It must also be observed, that the margin of the wire must not be exactly in the focus of the lens, since then the fringes disappear, for reasons which will be easily understood hereafter.

In order to establish the superiority of this mode of ob-

servation, it is sufficient to say that it readily exhibits the fringes which are formed by the light of a star of moderate brightness when an opaque body is interspersed, and even divides the shadow of a wire into light and dark stripes, when it is sufficiently fine, and far enough from the spectator; while even the shadow of such a wire would be imperceptible to the nicest eye if it were simply projected on a card. In order to see the fringes in the light of a star, it is necessary to employ a lens of a pretty long focus, such as a common spectacle-glass, of a focal length of a foot or two, because, if the glass were more convex, the light would be too much weakened; but in this case the magnifying power is not great, and it is impossible to observe very narrow fringes in this manner. The glass, as already observed, should be held for this experiment at such a distance from the eye as to appear wholly illuminated, and to throw all the light that it receives into the pupil.

In order to measure the breadth of the fringes, that is to say, the distances between the middle of the light and dark stripes, it is sufficient to employ a small moveable lens, carrying a fine wire in its focus, which serves as a point of comparison or index, and the displacement of which may be measured by a vernier, or by a screw, constituting the apparatus which is commonly called a micrometer. That which I have employed in all my experiments, and which was executed by Mr. Fortin, is provided with a plate of brass, which slides in two fixed grooves: this plate is perforated in the middle by a hole $\frac{4}{10}$ of an inch in diameter, to the edge of which is fixed on one side a single silkworm's thread, to serve as a mark, and on the other a little tube carrying the lens, which may be moved towards or from the thread till the proper focus is found. The plate, on which these pieces are fixed, is moved by a very accurate micrometer-screw: the breadth of the threads is exactly ascertained, and each revolution is divided by an index into 100 parts, so that in this manner the change of place of the mark may be measured within $\frac{1}{2500}$ of an inch. It will now be easy to understand how we can measure, for example, the distance between two dark stripes: we bring the thread in succession

to the middle of the first and that of the second, noting at each the division at which the index stands, together with the number of the entire turns, and by subtracting one of these measures from the other, we shall obviously obtain the required distance.

I might have begun by describing this mode of observation, but I was afraid of leaving some doubts respecting the simplicity of the causes of the phenomena; and I thought it better to describe the experiments as they had been made by GRIMALDI and by Dr. YOUNG, who received the shadows on a white screen. It is easy to perceive that the mode of observation makes no difference in the results: but it was better to demonstrate, in an incontestable manner, the diffraction of the light into the shadow, and the mutual influence of the rays of light on each other; and I have therefore deferred the description of the new mode of observation till I came to describe the new experiments that I had made.

By means, then, of two mirrors, we obtain a very striking example of the effects of the mutual influence of the rays of light, by the reunion of two beams that have been regularly reflected at their surfaces. We must not use silvered glasses, but rather glasses blackened at the back, in order that the second reflection may be destroyed; and metallic mirrors are still preferable. Having placed the two mirrors side by side, so that their edges are in contact, they must be brought very nearly into the same plane, but their faces must be made to incline a very little towards each other, so as to form an extremely obtuse angle. The magnitude of this angle must be regulated according to the interval which separates the images, and this interval must be small, if it is required that the fringes should be broad. But the point which requires the most particular attention is, that the mirrors should not project one beyond the other at the line of contact, for the difference of a few thousandths of an inch might be sufficient to prevent the appearance of the fringes; and this condition is best fulfilled by the help of the sense of touch, pressing by degrees that which appears to project, against the soft wax by which they are fixed in their places, and observing at the same time, by means of a

lens, if the fringes are properly formed. It would certainly be possible to construct an apparatus for performing this adjustment with facility, but great care would be required in its execution; and if the process which I have described is somewhat more troublesome, it has at least the advantage of requiring no other instrument than two small mirrors, either of metal or of black glass, which any person may easily procure.

In this experiment, as in those upon diffraction, it is necessary to employ the light of a single radiant point only; and in order that the fringes may be very distinct, it is necessary that it should be the more minute or the more distant as they are narrower. It is, however, of little consequence in what direction the light of this point falls on the mirrors. In order to see the fringes, we must go to a little distance from the mirrors, and receive directly the rays which they reflect on a lens of a short focus, behind which the eye must be held in such a manner as to see all its surface illuminated. The fringes will then be discoverable in the space illuminated by both the mirrors, which is easily distinguished from the neighbouring space by the greater brightness of the light.

The fringes present a series of bright and dark stripes, parallel to each other, and at equal distances. In white light, when care is taken to make the fringes broad enough, they are embellished by very vivid colours, especially towards the middle of the space; for at a distance from the middle the colours gradually become fainter, and disappear altogether after about eight alternations. In a more homogeneous light, such as may be obtained by means of a prism, or of certain red glasses, the number of fringes becomes much greater, each of them, however, consisting only of dark and bright spaces of the same colour: and by employing a light as purely homogeneous as possible, the phenomenon is obtained in the greatest possible simplicity; and it is in this form that it will be most convenient to study it in the first instance. It will afterwards be easy for us to understand the appearances which are observed in white light by the superposition of bright and dark stripes of each of the species of coloured rays that compose it.

The direction of these stripes is always perpendicular to a right line, supposed to join the two images, at least in the space enlightened by the rays that are regularly reflected, whatever the direction of that line may be with regard to the line of contact of the mirrors; which is sufficient to show that they do not depend on any influence exerted by the edges of the mirrors on the light that passes near them. Besides, by altering the angle which the mirrors form with each other, we may separate the images so far from each other, as to cause the rays which form them to be reflected at too great a distance from the edges in contact, to allow us to suppose that any sensible influence can be exerted on the light by these edges.

The middle stripe is always brilliant, like that of the fringes which are seen in the shadow of a narrow body, or those which are obtained from a plate furnished with two fine parallel slits very near each other. This bright stripe is situated between two dark stripes of the deepest black, when we employ homogeneous light: each of these is followed by a bright stripe, and these again are succeeded by dark stripes, and so forth. The dark stripes are still of a dark black in the fringes of the second and third orders; but, in proportion as we go further from the middle, the tint is less decidedly black, because the light employed is never perfectly homogeneous.

It is sufficient to compare the three first pairs of dark stripes with the light afforded by a single mirror, to convince ourselves that they are much less enlightened, and that in the places which they occupy, the addition of the rays of one of the mirrors to those of the other, instead of increasing the light, actually *produces darkness*. It is easy to make this comparison by looking successively at the black stripes, and at the enlightened parts situated to the right and left of the space which is doubly illuminated, and in which the fringes are found. If we were apprehensive that the contrast with the bright stripes in the neighbourhood might occasion some fallacy in this respect, it would be sufficient to place the thread of the micrometer successively in one of the black stripes near the middle, and in the space enlight-

ened by a single mirror; for it will be much more visible in this latter situation than in the former, especially if the room is well darkened, and if all proper precautions have been taken to exclude all the light but that which is the subject of the experiment.

It is then completely demonstrated, that, in certain cases, light added to light produces darkness. This capital fact, which did not escape GRIMALDI, although NEWTON seems to have been ignorant of it, had been sufficiently proved in our own times by the experiments of Dr. YOUNG; but the phenomena which I have related afford perhaps clearer evidence of its truth, because the dark stripes that it exhibits are, in general, blacker than those which are produced by diffraction, properly so called, and because it supersedes every idea of the operation of a *diffractive* force, which might be supposed capable of expanding the beams of light in some places, and condensing them upon others, since the effect is here exhibited by rays which have been regularly reflected.

[To be continued.]

The Life of Edward Jenner, M.D., LL.D., F.R.S., Physician Extraordinary to the King, &c. &c.; with Illustrations of his Doctrines, and Selections from his Correspondence. By John Baron, M.D., F.R.S. London, 1827.

THE close of the eighteenth century was rendered memorable by the promulgation of a fact in the natural history of man, the consequences of which have been, and continue to be, of a magnitude quite incalculable. In every habitable part of the earth its effects have been felt, and are still to be witnessed. To the physiologist, studious of the analogies that subsist among the different classes of animals, it opened a wide field of interesting research. The physician viewed it as giving him a power over disease greater than any which medical skill had brought to light during a period of two thousand years. To the eye of the political economist, it offered the prospect of a rapid and momentous increase in the population of the country. The philanthropist hailed it as affording the certain prospect of diminishing the sum of human misery; and to those who were content to look on it with other eyes than those of science, a spectacle was pre-

sented, not less sublime than when the angel of the Lord stayed the pestilence from the people of Israel.

The private and public life of the man who thus enlarged the bounds of human knowledge, and circumscribed the limits of human suffering, must possess interest with all classes of readers, and that of no common kind; and, in this assurance, we enter on the review of the work before us. It has every claim upon our attention. Dr. Baron was for many years the intimate friend and companion of Dr. Jenner. The whole of his notes and correspondence was unreservedly placed by the executors in Dr. Baron's hands. Bred in the same profession, and zealous in the same cause which occupied the time and the thoughts of Jenner, Dr. Baron was certainly the fittest man to whom the task could have been intrusted of delineating his character and opinions. The manner in which that task has been executed demands, in the outset, our warmest approbation. No pains have been spared to collect information. No marks of hurry are perceptible in the filling up of details. Although the portion of the work dedicated to abstruse medical discussions may perhaps prove irksome to the general reader, and will certainly appear to all out of fair proportion to the strictly biographical part, yet it would have been difficult for the author, in any other way, to have elucidated fully the doctrines and practice which it was the great object of Dr. Jenner's life to inculcate. It is to be regretted that the work, in its present state, is imperfect. Another volume, probably of equal size, will be required to complete it. The present gives us only the early history of Jenner, and the five first years of vaccination. It closes at the most brilliant epoch of Jenner's life, the very zenith of his career—Autumn, 1803; when his name had become known to the remotest corners of the earth, and when, from foreign countries, he was receiving every mark of honourable distinction, and reaping, in his own, a rich harvest of professional reputation, with the more substantial benefit of ten thousand pounds, granted by parliament. The reasons which the author adduces for publishing the first, without waiting for the completion of the second part, are such as must command our assent;—an earnest hope, that a more perfect acquaintance with the views of Jenner may restore, and even increase, public confidence in the virtues of cow-pox; and the feeling, that his own professional avocations, and the disarranged state of the voluminous documents which he has in his possession, will prevent him, for some time to come, from completing the arduous duty which he has undertaken.

We think it will be for the convenience of our readers, if we separate the historical part of the work from the purely speculative and physiological discussions with which it abounds. This arrangement will have the additional advantage of enabling us to offer, hereafter, a few criticisms on the principal topics connected with vaccination, on which the judgment of the world is now, in some measure, divided. Dr. Edward Jenner, born at Berkeley, in Gloucestershire, on the 17th May, 1749, was the third son of the Rev. Stephen Jenner, A.M., the vicar of the parish. His mother was the daughter of the Rev. H. Head, a former vicar of Berkeley. Jenner's father possessed, besides his church preferments, considerable landed property. He did not long survive the birth of his son Edward, whose early years were passed under the roof and guidance of his elder brother. His instructors in classical literature were the Rev. Mr. Clissold of Wootton-under-Edge, and the Rev. Dr. Washbourn of Cirencester. In the elements of surgery and pharmacy he was instructed by Mr. Ludlow, an eminent surgeon of Sodbury, near Bristol. At the age of twenty-one he went to London, where his professional studies were completed under the direction and instruction of the celebrated John Hunter, in whose family he resided for two years. We are not favoured with any anecdotes of the early years of Jenner; but it is clear, from the whole tenor of his correspondence, that he early imbibed a keen relish for a country life, which, ere his arrival in London, had given a tone to his thoughts and studies. John Hunter seems fully to have appreciated the peculiar merits of his pupil. Through his recommendation, Jenner was appointed to arrange the cabinet of natural history, the fruits of Captain Cook's first voyage, and was even offered the situation of naturalist to the second expedition. When he returned to Berkeley, and entered upon the active duties of his profession, John Hunter employed him in making a variety of experiments, for which his rural predilections especially fitted him. We must extract one of John Hunter's letters to Jenner, which, while it evinces the confidence he placed in the talents and accurate observation of his young friend, places also, in a strong point of view, the extraordinary powers of the writer's mind, which seems to have grasped, at once, the whole extent of animated nature.

"Mr. Hunter to E. Jenner.

"Dear Jenner,—I received your salmon and very fresh, and just examined enough to want another, but will wait till another season. If I

was to have another, it should be one that had just spawned; I will take a cock salmon when you please.

"If you catch any bats let me have some of them; and those you try yourself, open a hole in the belly, just size enough to admit the ball; put the ball down towards the pelvis, and observe the heat there; then up towards the diaphragm, and observe the heat there; observe the fluidity of the blood; do all this in a cold place. Extraneous fossils are all vegetable and animal productions, found in a fossil state. See if you can catch the number of pulsations and the frequency of breathing in the bat, without torture. If the frost is hard, see what vegetables freeze; bore holes in large trees, and see whether the sap runs out, which will show it is not frozen. I am afraid you have not a proper thermometer. I will send you one.

"Your very much obliged servant,

"J. Hunter."

The friendship and example of such a man as John Hunter must have had considerable influence over the mind of Jenner. They stimulated him to prosecute actively different branches of natural science; and the three first chapters of Dr. Baron's work present us with a very pleasing picture of the various pursuits in which he occupied himself from the age of twenty-three to that of forty-five. He devoted himself, with an earnestness truly admirable, to the duties of his profession. For twenty years he practised, with great reputation, as a surgeon and apothecary. On one occasion he was sent for to Gloucester, where he successfully performed a difficult and delicate operation. He obtained the degree of M.D. from St. Andrew's, in 1792, to which honour he was well entitled. He was, in truth, an excellent physician. A long letter to his friend, the Rev. Mr. Clinch (published at p. 88), gives us a sketch of his views concerning fever, which are well worthy of his name. He appears to have the merit of first detecting the pathology of that formidable disease the angina pectoris. His notions concerning tuberculous disorganizations, and the disordered conditions of the lymphatic system generally (alluded to at p. 99), are very ingenious, and serve to display the extent and variety of his powers. As a pharmaceutist, he is entitled to some praise, having, early in life, introduced the mode of preparing tartar emetic, from the glass of antimony, which is now adopted by the London college. Those who are curious in such matters, may perhaps trace to this little circumstance the peculiar attachment to this remedy which Dr. Jenner shewed in the latter periods of his life.

While his mind was thus occupied in the active duties of an arduous profession, it is delightful to contemplate the ardour with which he pursued so many other objects of re-

search. He made some curious experiments in horticulture (p. 76). He was devoted to the study of mineralogy (p. 59), and spent a great deal of time in investigating the organic remains with which the oölitic and lias formations of the districts in his neighbourhood abound. Occasionally he courted the Muses; and some very amusing specimens of his poetical talents are afforded us at pp. 25 and 542. His greatest enjoyment, however, was in the study of Ornithology, which he pursued with uncommon industry, and no small share of success. John Hunter urged him to investigate the habits of the cuckoo; and in the year 1788, his admirable paper on that subject was presented to the Royal Society. He clearly proved the fact of the *fostering* of the young bird, and threw out the ingenious suggestion, that it was a provision of nature rendered necessary by the short residence of the parent bird in this country. This essay was favourably received by the scientific world, and procured him, the following year, admission into the Royal Society. The migration of birds occupied many of his leisure hours. In his early years, at the instigation of John Hunter, he made a variety of observations on the hedgehog, some account of which still exists; but, alas! not one of his replies to his patron's numerous letters, and still more numerous queries are given; and from what is stated at p. 99, there is too much reason to fear they have all perished.

In 1788, Dr. Jenner married Miss Catharine Kingscote, a lady of elegant manners and strong understanding, to whom he was greatly attached. In domestic life he was distinguished by an amiable disposition, which rendered him an universal favourite. He was the founder and principal supporter of two societies, where he and his professional brethren discussed medical subjects, and cultivated each other's acquaintance. These he called, quaintly enough, the Convivio-Medical, and the Medico-Convivial Societies. To the former, which was held at Alveston, a village about ten miles from Bristol, we shall have occasion to refer hereafter. The following account of Jenner's appearance is given by one of his earliest friends. "When I first saw him, it was on Frampton Green. I was somewhat his junior in years, and had heard so much of Mr. Jenner of Berkeley, that I had no small curiosity to see him. He was dressed in a blue coat and yellow buttons, buckskins, well-polished jockey-boots, with handsome silver spurs, and he carried a smart whip with a silver handle. His hair, after the fashion of the times, was done up in a club, and he wore

a broad-brimmed hat. His height was rather under the middle size. His person was robust, but active and well formed. In his dress he was peculiarly neat; and every thing about him showed the man intent and serious, and well prepared to meet the duties of his calling. We were introduced on that occasion, and I was delighted and astonished. I was prepared to find him an accomplished man, and all the country spoke of him as a skilful surgeon, and a great naturalist, but I did not expect to find him so much at home on other matters." His society was much coveted and prized. "He had," says his biographer, "a singular and happy union of scientific and original observation, with the playfulness, and mirth, and wit, of familiar intercourse. His imagination was singularly vivid, and he had a peculiar felicity, even in common conversation, of clothing his remarks in the gay and lively colours of poetry. He was a collector of old pictures, very fond of music, could play on the violin and flute, and would now and then sing one of his own ballads." Such a man was Dr. Jenner, the discoverer of Vaccination. It is time that we give our readers some idea of the progress of his own mind towards that great event. The work before us is rich in illustrations of this interesting topic.

In the green fields and shady lanes of the beautiful vale where he resided, Jenner early obtained an acquaintance with the popular rumours concerning cow-pox; but the following circumstance first rivetted his attention. While he was an apprentice at Sodbury, and probably about sixteen, a young woman applied for advice. The subject of small-pox was mentioned in her presence. She immediately observed, "I cannot take that disease, for I have had Cow-pox." He repeatedly mentioned this circumstance to John Hunter, who advised him, we are told, to try, but certainly gave him no great encouragement. In none of his letters does he ever inquire, even in a postscript, how the said trials with the Cow-pox proceed. We may fairly presume, therefore, that John Hunter with all his learning, disregarded the matter.

Of the precise amount of the popular opinions long prevalent, concerning cow-pox and its influence, we shall say something presently; but the impression which they made upon young Jenner was obviously very strong. In 1775, he began to view it as a matter of scientific investigation, and from that time, until 1796, when he made his first decisive experiment, it was probably never altogether out of

his mind. Eighteen years prior to that event, that is to say, in the month of May, 1780, he had a glimpse of the truth, and of the reputation that awaited him. He was riding with his friend Gardner, on the road between Gloucester and Bristol, when he briefly sketched out the course which he hoped to pursue, and which he ultimately lived to accomplished. At the meetings of the Alveston club, formerly mentioned, he frequently brought forward the subject of cow-pox, and earnestly recommended his friends to prosecute the inquiry; but so little did they think of it that they voted cow-pox a great bore, and threatened to expel Jenner if he continued to harass them with so unprofitable a subject. (p. 48.) To appreciate fully the merits of Jenner, as the discoverer of vaccination, it should be remembered, that he made no secret of his belief in its virtues. In 1788, he took with him to London, a drawing of the casual disease, as seen on the hands of the milkers, and showed it to Sir Everard Home, and others. John Hunter had often mentioned the fact in his lectures. Dr. Adams heard of the cow-pox, both from Mr. Hunter and Mr. Cline; and in his treatise on the morbid poisons, published in 1795, three years prior to the public announcement of Jenner's views, mentions the principal facts then known concerning it. Many other persons, too, (Dr. Haygarth for instance,) were acquainted generally with its alleged powers; yet no one had the boldness to stake his fame and character upon this basis but Jenner. To what are we to ascribe this?—not to the ambition of youth, for Jenner was forty-seven, and had a comfortable independence when he first published concerning cow-pox;—not to mere vanity, for Jenner was habitually of modest and retiring manners; but it is to the confidence which science gave, that we are indebted for the introduction of vaccination. Jenner had paid much attention to epizoötic maladies, and the principles which he had thence deduced emboldened him to persevere in an effort, in which he got no positive help and scarcely any encouragement. He was thoroughly sensible, however, of the heavy responsibility which he was incurring. His feelings indeed, throughout his whole life, with reference to this great event, do him the highest honour; and we are sure that our readers will be gratified by the picture which he has himself left us of his own mind, soon after the successful issue of his first and most important experiment.

"While the vaccine discovery was progressive, the joy I felt at the prospect before me of being the instrument destined to take away from

the world one of its greatest calamities, blended with the fond hope of enjoying independence and domestic peace and happiness, was often so excessive that, in pursuing my favourite subject among the meadows, I have sometimes found myself in a kind of reverie. It is pleasant to me to recollect that these reflections always ended in devout acknowledgments to that Being from whom this and all other mercies flow."—(p. 140.)

The 14th of May, 1796, is usually assigned as the birth-day of vaccination. At Berlin, it is still annually celebrated. On that day, James Phipps, a boy eight years old, was vaccinated by Dr. Jenner; on the first of July following, he was inoculated with small-pox. We give the rest in the words of Jenner himself: "Listen now to the most delightful part of my story. The boy has been inoculated for the small-pox, which, as I ventured to predict, produced no effect; I shall now pursue my experiments with redoubled ardour." Most zealously, indeed, did he follow up this auspicious beginning. Early in 1798, another opportunity occurred of pursuing his inquiries, and he now prepared for publication. Dr. Baron is silent as to the cause of the non-appearance of his paper among the 'Transactions of the Royal Society;' the deficiency, however, is supplied by Mr. Moore in his History of Vaccination. (p. 20.) He was admonished not to present it, lest it should injure the credit he had established for himself among scientific men by his Essay on the Cuckoo! The work, having first been carefully scrutinized by a knot of his intimate associates, was at length published, about the end of June, 1798. It was entitled "An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, a Disease discovered in some of the Western Counties of England, particularly Gloucestershire, and known by the name of the Cow-pox." The object of the work was two-fold. First, to announce the security against small-pox which the true cow-pox gives; and secondly, to trace the origin of that disease in the cow, to a similar affection of the heel of the horse. An unassuming tone pervades this first Essay on Vaccination which is very striking. The author modestly suggests the probability of its usefulness in certain cases which he points out, but he never hints at the idea of its ultimately exterminating the small-pox.

The late Mr. Cline, a steady friend and admirer of Jenner, who had corresponded with him, several years before, on the subject of cow-pox, was the first person in London who adopted the new practice. Two months prior to the publication of his book, Dr. Jenner came to town,

and he remained there for three weeks after its appearance; yet strange to say, with all his own efforts and those of his friends, he was unable, during that long period, to procure *one* person in the metropolis on whom he could exhibit the vaccine disease. Mr. Cline's acknowledged reputation gave the bent to the public mind, and before the year was expired, vaccination had made rapid advances in general esteem.

Mr. Cline and Sir Walter Farquhar now anxiously pressed Dr. Jenner to settle as a physician in town, and it is certainly to be regretted that he did not act upon their advice. Much of the evil which afterwards ensued, and some portion of that professional jealousy which broke out and tarnished the rising glory of vaccination, would probably have been checked, had Jenner been upon the spot. His presence would have directed the public judgment, and nipped in the bud the insidious designs of some who took a prominent part in the extraordinary scene which presently displayed itself. A rush towards vaccination succeeded to, and strangely contrasted with, the apathy and distrust with which it had hitherto been viewed. It was not only recommended, but practised, by persons of all ranks and conditions, without any knowledge of what they were really doing. That this was in some degree encouraged by the confident tone of those, whose professional experience should have taught them caution in all matters relating to the conjectural art of medicine, cannot indeed be denied. Even Jenner himself is not free from this censure. Within two years from the promulgation of his discovery, we find him employing such strong expressions as these: "The scepticism that appeared even amongst the most enlightened of medical men when my sentiments on the important subject of the cow-pox were first promulgated, was highly laudable. To have admitted the truth of a doctrine at once so novel and so unlike any thing that ever had appeared in the annals of medicine, without the test of the most rigid scrutiny would have bordered on temerity; but now when that scrutiny has taken place, not only amongst ourselves, but in the first professional circles in Europe, and when it has been uniformly found, in such abundant instances, that the human frame, when once it has felt the influence of the genuine cow-pox in the way that has been described, *is never afterwards, at any period of its existence, assailable by the small-pox*, may I not with perfect confidence congratulate my country and society at large, on their beholding, in the mild form of the cow-pox, an antidote, that is ca-

pable of *extirpating* from the earth a disease which is every hour devouring its victims, a disease that has ever been considered as the severest scourge of the human race!"*

The early career of vaccination, though very brilliant, was clouded by some disappointments. In the first experiments at the Small-pox Hospital, the vaccine lymph became contaminated with small-pox, which the physician to that establishment could not be made to believe. Some weak persons, with Dr. Moseley at their head, assailed it with the shafts of ridicule; but Dr. Jenner was more annoyed by the conduct of some of those who avowed themselves friends to vaccination, than by all the efforts of its open enemies. Professional envy was at work, and a regular attempt was made to wrest from Jenner's brow the laurels which he had so fairly won. Instead of strictly questioning the accuracy of some of Jenner's views, his opponents joined in the loudest praise of vaccination, but allowed to Jenner only just sufficient merit to entitle him to the appointment of extra-corresponding physician to a vaccine institution, with the privilege of recommending patients, by proxy, on payment of one guinea per annum. (p. 361.) The whole conduct of the physician who stood prominently forward on this occasion, is animadverted on, in very strong terms, by Dr. Baron, who with the honest warmth of a biographer, is indignant at the slight thus cast upon the hero of his history. On this painful part of the subject, however, we will not dwell longer, but proceed to record the gratifying tributes to his merit which Dr. Jenner received from other quarters. A strong testimony of confidence in the virtues of the Cow-pox was signed so early as the summer of 1799, by thirty-three of the most eminent physicians, and by forty distinguished surgeons of the metropolis. Early in 1800 the Duke of Clarence exerted himself greatly in the cause; and in the month of March that year, Dr. Jenner was successively introduced to the Duke of York, the King, the Prince of Wales, and, lastly, to the Queen, all of whom received him with marked attention, and expressed the interest they felt for the success of vaccination.

By these and similar proofs of the public feeling, individual clamour was at length hushed; and in the propagation of Cow-pox to distant regions, Jenner quickly found ample employment for his time and thoughts. Dr. Baron gives

* Jenner's "Continuation of Facts and Observations." 1800.

copious details concerning the mode in which this object so dear to his heart was effected ; as also concerning the persons chiefly instrumental in carrying his views into effect. Many of these details possess great interest, especially those which concern the introduction of cow-pox into the great continent of Asia (p. 420); but we have no space for them here. Suffice it to say, that the late Dr. Gregory had the merit of introducing vaccination into Scotland, through the medium of Sir Matthew Tierney. Dr. Waterhouse undertook to introduce it into America, and by his perseverance and talents, fully succeeded in doing so about the year 1800. The earliest supporter of vaccination on the continent of Europe was Dr. De Carro of Vienna, whose exertions in the cause are beyond all praise. To him our Indian possessions are indebted for the introduction of the vaccine. Dr. Sacco, of Milan, distinguished himself not merely as an active propagator of the new practice in Italy, but by his pathological inquiries into the origin of cow-pox.

Dr. Baron dedicates one chapter of his book to a detail of the events connected with the first parliamentary grant to Dr. Jenner. A committee was appointed to examine and report upon Dr. Jenner's claims for public remuneration. Witnesses pro and con were examined. It was stated, that a farmer of the name of Jesty had actually inoculated his wife and children with cow-pock matter in 1774, and that a Mrs. Rendall had caused five of her children to play with the teat of a cow to secure them from small-pox; but this, as Dr. Baron observes, never advanced the cause of vaccination beyond what popular rumour had already done. Dr. Jenner's merit consisted in this, that he divested popular tradition of its obscurity and uncertainty, and gave the aspect of science to what was formerly vague and valueless. A pretty illustration of that kind of merit which belongs to Jenner is given by Dr. Baron, at p. 562. A fish was preparing for dinner in the kitchen of a medical man, and was accidentally placed on a table in connexion with two metals. The fish was thrown into convulsions. The doctor recorded the circumstance, published an account of it, and there the matter ended. The same sort of thing afterwards occurred in the laboratory of Galvani. He set himself to *investigate* the phenomenon, and the genius which this effort displayed was soon rewarded by a rich harvest of discovery. But to return to the Committee of the House of Commons, whom we left discussing Dr. Jenner's claims. They considered him well entitled to 20,000*l*., but Mr. Bankes, the Joseph Hume

of those days, would not agree to more than ten. A vote for ten thousand pounds in favour of Dr. Jenner, passed the House on the 2d June 1802, by a majority of three.

It is a remarkable circumstance, that very few real improvements in the practice of vaccination have been introduced since the discovery was first announced. One of the most important was the practicability of propagating the disease by scabs, for which we are indebted to Mr. Bryce of Edinburgh. Of the value of this addition to our knowledge concerning cow-pox, Dr. Jenner was fully sensible, but he attached comparatively little importance to the *test*, as it is called, recommended by that gentleman, and known to vaccinators by his name. In this opinion we think Jenner fully borne out, as we have never been able to convince ourselves that that supposed test furnishes any evidence of the *degree* to which the constitution is affected. This still remains a great desideratum. A very effectual mode of preserving lymph for the use of distant countries, was invented by two German physicians, and is detailed at p. 430.

Honours began to pour in upon Dr. Jenner from the year 1801. The Dowager Empress of Russia sent him, in that year, a ring set in diamonds. The Royal Society of Madrid elected him an honorary member ; &c. &c. With a notice of these blushing honours Dr. Baron's first volume concludes. It is far from our intention to anticipate the contents of that which is to come, but it may be satisfactory to our readers, to have one or two dates, by way of filling up the picture. In 1807 parliament reconsidered its former vote, and granted to Dr. Jenner an additional sum of twenty thousand pounds. In the following year, vaccination was taken under the protection of government. The National Vaccine Establishment was at first placed under the immediate direction of Dr. Jenner, but difficulties ensued, and Dr. Jenner resigned. During the latter years of his life, he continued to devote a great deal of his time to the subject of vaccination, but he did not publish any thing of much importance concerning it after the period to which Dr. Baron brings down his life. He died at Berkeley, in February 1823, suddenly, of apoplexy, in the seventy-fourth year of his age. A statue has been erected to his memory in his native county, but, hitherto, no adequate testimonial of national approbation has been bestowed. An anxious wish was expressed by many of the admirers of his genius, that his remains should be deposited in Westminster Abbey,

with the distinguished of the land, and government were well disposed to accede to their wishes, but for some reason it was thought unadvisable, and his body lies in the chancel of the parish church in Berkeley.

We have now gone through the task which we first proposed to ourselves,—that of giving a brief sketch of the principal events in Dr. Jenner's life. It remains that we offer a few criticisms on the different speculative topics discussed, often at great length, in Dr. Baron's work; and for the sake of brevity, as well as perspicuity, we shall take them up in the following order:—1. The antiquity of small-pox. 2. The common origin of human and epizoötic maladies; the identity of cow-pox and small-pox; and the equine origin of the former. 3. The possibility of exterminating small-pox. 4. The causes of vaccine failure.

1. Of the antiquity of small-pox.—This is one of the thorny points in medical literature, with which Dr. Baron boldly grapples, and a whole chapter is devoted to its discussion. It is not made to appear, however, that the opinions here delivered were really those of Dr. Jenner. We would suggest, therefore, to the author, the propriety of separating these details, in a subsequent edition, from the body of the work, and of throwing them, with some others of a similar kind, into an appendix. This will be a great relief to the general reader, without impairing the value of the work to the inquiring physician. Dr. Baron supports the opinion of the late Dr. Willan, that the small-pox is a disease of great antiquity, and that it is to be traced in the earliest writings of the Hebrews and Greeks. He gives us an abstract of the plague of Athens, as described by Thucydides, and adds, "in this quotation from the original, will be found, if I mistake not, as accurate an account of the leading symptoms of variola as could possibly be expected from any historian not medical." With all deference, we do think the author is mistaken here. We know of no disease, the prominent characters of which could be so easily described by an unprofessional man, as small-pox. But it is clear from what the historian says, that he watched the symptoms of the complaint, not superficially, but very closely, and he describes, in our judgment, with great accuracy, a petechial typhus, or, perhaps, cynanche maligna. According to Dr. Baron, small-pox was seen by Hippocrates, and commented upon by Galen, but it requires a great stretch of ingenuity to trace in their writings any of the peculiar features of this disease. Rhazes, the Arabian, the first *ac-*

knowledged writer on the small-pox, struggles hard to prove that Galen had seen it; but even he, with all his enthusiasm for his master, was sadly puzzled to account for the unwonted brevity and inaccuracy of his description. This, we think, is of itself decisive of the question; but when we further call to mind the recorded opinions of the best modern writers who have devoted their attention to the history of physic, we mean Friend and Mead, we confess we have no hesitation whatever on the subject. We regret that Dr. Baron should have supported this side of the question, as by some it may be considered (however undeservedly) as an impeachment of the general accuracy of Dr. Jenner's views.

The first notices of a disease which exhibits the well-marked features of small-pox, are to be met with in the historical writings of Procopius, who flourished during the reign of Justinian the First. The obscurity of its origin, the difficulty of its cure, the universality of its devastations, and above all, the complete immunity from second attacks, bespeaks this epidemic to have been truly small-pox. It began A. D. 544, and was reported to have been brought to Constantinople from Æthiopia. This corresponds closely with the era commonly assigned in medical books to the first appearance of small-pox; viz., A. D. 568, when the Abyssinian army, under Abrahah the viceroy, besieged Mecca. Without going, therefore, deeply into historic details of little general interest, we may say that small-pox first appeared in the East about the middle of the sixth century.

2. The next topic which we have set down for consideration, is, the common origin of human and epizoötic maladies. This curious but uninviting branch of medical science was a favourite subject of speculation with Dr. Jenner; and, accordingly, repeated allusions are made to it in the work before us. In the first volume of the Transactions of the Medical and Chirurgical Society of London, there is a paper by Dr. Jenner, on the distemper in dogs, a contagious disease *not* communicable to man. There are certain other epidemic maladies, however, which affect equally man and the brute creation, of which the most important is, according to Dr. Jenner, that which in its most malignant form we call small-pox, and in its milder forms chicken-pox, swine-pox, and cow-pox. It is, undoubtedly, a matter, not of mere curiosity, but of serious reflection, that in their nomenclature of certain diseases, the vulgar seem to have acknowledged this doctrine of a common origin to human and epizoötic epide-

mics. How else did the terms chicken-pox and swine-pox creep into our language. "In Bengal," says Dr. Jenner, (p. 237) "the poultry are subject to an epidemic eruption, which kills them by hundreds. The natives have only one name for this disease and the small-pox, *gootry*." "A traditionary account is handed down to us by the Arabian physicians, that the small-pox was originally derived from the camel." (P. 522; Letter from Dr. Jenner to H. R. H. the Duke of Clarence.) We have an account at p. 238, of some children who were inoculated at Madrid in March 1804, by order of the king of Spain, with a goat-pock. We are not favoured, however, with the result of the test-trials, or contra-proofs as they are called by the professor Heydeck.

"It seems certain, then," says Dr. Baron (p. 243), "that there are at least four animals, namely, the horse, the cow, the sheep, and the goat, which are affected by a disorder communicable to man, and capable of securing him (to a certain degree at least) from what appears to be a malignant form of the same disease." Into the many illustrations given of this position we cannot here, of course, enter; but our readers will gather from the remarks now offered some ideas of the reasoning by which Dr. Jenner was induced to *experiment* on the influence of cow-pox. The sum of his doctrine was briefly this. He not only considered small-pox and cow-pox as essentially the same disease, but he imagined "that the former was only a malignant variety of the latter, the parental root being the cow-pox." (p. 357.) This notion of the identity of cow-pox and small-pox, so frequently urged by Jenner, and so pointedly marked by his phrase *variolæ vaccinæ*, has been much questioned in later times, and a few observations upon it may not be misplaced.

Diseases that mutually produce each other are clearly referrible to the same source. The identity of swine-pox and small-pox is, therefore, generally admitted. Dr. Adams, however, entertained the opinion, that by successive inoculations with a mild matter, a *permanent* modification of small-pox might ultimately be obtained, and he actually persuaded himself that he had succeeded in the attempt. He called the disease *pearl-pox*, and it appears to have been intermediate between cow-pox and distinct small-pox. Some notion of the same kind probably influenced Jenner, when, in 1789, he inoculated his eldest son with the matter of swine-pock. The child took a mild and modified disease, and resisted the future action of variolous matter. Dr. Jenner seems to have retained the same opinion as late as

1799, for when detailing to Dr. Woodville a similar case, he says, "it would be unfair to draw positive conclusions from such scanty precedents; but yet they lead one to hope, that a mild variety of the small-pox might thus be actually created." The idea of a disease having fixed and permanent characters, less virulent than small-pox, and more obviously energetic than cow-pox, had often passed through our minds, but until we read Dr. Baron, we were unaware of the advance that had been made towards determining the point. The cow-pox, however, is differently circumstanced. It never produces small-pox, and small-pox never produces it, at least in man; and, therefore, it is certainly questionable whether these two disorders are essentially the same;—in other words, whether the cow-pox be a permanent, as the swine-pox is a temporary, modification of variola. In favour of such an opinion, Dr. Jenner appears, as far as we can judge from the scattered discussions in Dr. Baron's book, to rely principally on the two following arguments:—1. The cow-pox in the cow is not a local, but a constitutional, and even sometimes a malignant disease. (p. 352.) 2. The cow-pox contaminated by small-pox will sometimes lose its virulence, cease altogether to produce eruptions, and at length become almost assimilated to the true vaccine character. (p. 245.) Upon the whole, the question of the identity of cow-pox and small-pox may be considered as still undecided. The general voice of the profession is, we think, rather against, than in favour of the doctrine.

The straits and difficulties of this inquiry, however, lie still before us. We have yet to investigate the alleged connexion of cow-pox with the disorder called the grease in horses. In Jenner's first essay, this subject is brought prominently forward, and not only is the identity of the two affections maintained, but it is confidently stated, that the cow-pox *never* occurs in dairy countries, except where there is access to horses. We cannot help thinking that the great stress thus laid upon the equine origin of cow-pox was somewhat injudicious. The facts bearing upon the question were then only imperfectly known to Jenner. (See p. 243.) The doctrine itself had no obvious connexion with the great practical truth which he laboured to establish. It could not tend in any degree to strengthen public faith in the virtues of the cow-pox, and it certainly gave a handle to the opposers of vaccination, of which they were not slow to avail themselves. It gave occasion to endless controversies and bickerings. We are told (at p. 584), that in 1803, all the principal medical men in

London were adverse to the opinion. Later observations have, indeed, established the identity of the grease and the cow-pox, but they have shewn, at the same time, the incorrectness of some of Jenner's first ideas on the subject. The following extract of a letter from Dr. De Carro, of Vienna, to Dr. Monro of Edinburgh, written in 1825, (to be found in the Edinburgh Journal of Medical Science), may be quoted as a fair illustration of the importance of the doctrine, and of the improved notions concerning it. "The source of our cow-pox (at Vienna) says Dr. De Carro, is partly British, and partly originating from the grease of a horse at Milan, without any intervention of a cow. The effect was so similar in every respect, that they were soon mixed;—that is to say, it was impossible to tell, after several generations, and in the hands of innumerable practitioners, what was equine and what was vaccine. The whole British settlements in India have been *equinated;* for the first liquid drop which I sent twenty-five years ago to India was the second generation of Milanese equine or greasy matter, transplanted at Vienna. You know, by frequent reports from the East, that the practice there is upon the best footing." But we fear we shall tire our readers if we prosecute further the alleged connexion of human and epizoötic maladies. We shall pass on, therefore, to another subject.

3. In several parts of Dr. Baron's work, the doctrine is taught, that cow-pox possesses powers adequate to the complete extirpation of small-pox from the face of the earth. We have already given an extract from the earlier writings of Jenner, in which this sentiment is strongly expressed. We may point out one or two other passages in which the same thing occurs. Dr. Jenner's petition to the house of commons, in the year 1802, contains the following paragraph:—"The said inoculation hath already checked the progress of the small-pox, and, *from its nature, must finally eradicate that dreadful disorder.*" Dr. Baron, in p. 259, has the following remark: "But the property, of all others, which peculiarly distinguishes the variolæ vaccinæ from small-pox, and *which would enable us to banish the disease entirely*, is, that they are not communicable by effluvia." This notion very naturally made a strong impression upon the public mind, and contributed, in no small degree, to encourage that ardour in the cause of vaccination which was so honourable to the country. We cannot, however, on the grounds of physiology, or of plain common sense, understand how such an event could be brought about by the means suggested,

Mankind vaccinate their children to escape the dangers of small-pox: when, by the gradual lessening of the disease, such dangers were removed from view, those who know any thing of human nature, will at once perceive, that the preservative practice would fall into neglect; and so in fact it has ever proved in countries where government does not enforce vaccination under penalties. So much for the common sense view of the question. But we suspect the correctness of the physiological principle on which the idea was upheld. It assumes, in the first place, that small-pox in man arises invariably from contagion, a position which we are strongly inclined to favour, but which is at least open to discussion. The grease in horses, and the pock in cows, are universally admitted (we believe even by Jenner himself) to be sometimes of spontaneous or sporadic origin. How otherwise did it happen that, from 1780 to 1796, Jenner could meet with no opportunities of prosecuting his inquiries into the cow-pox? Now, if we admit the identity of small-pox, grease, and cow-pox, it is difficult to understand the extraordinary diversities in their mode of origin. If, on the other hand, to establish the possibility of exterminating small-pox, we adhere to its propagation by contagion alone, the identity of the two diseases is certainly brought into doubt.

But further, to show the correctness of this position of Dr. Jenner, the state of small-pox in foreign countries is frequently brought forward. It is confidently urged, that by salutary regulations, and strong confidence in the public, the desirable end proposed by Jenner has actually been accomplished, and that in some districts abroad small-pox is no longer to be seen. We strongly suspect, however, that this is what logicians call an *argumentum ab ignoto*. In Sweden, one of the countries often referred to, they had a smart attack of epidemic small-pox in 1824, affecting, in many cases, those who had been vaccinated; which, though it did not in the least shake their faith in vaccination, yet proved very effectually the impossibility of exterminating the disease. Within the last year or two, the Dutch government sent over some young doctors to this country, to inquire concerning vaccination, in consequence of meeting with difficulties of a like kind. The medical men at Paris, who were long very sceptical on the subject of small-pox after vaccination, understand it now as well as we do here. We do not mention these things out of any distrust of vaccination, but merely that the superior good management of foreign powers, and the greater good fortune of those who live under them (with

reference to small pox), should not unfairly be cast in our teeth.

4. We have now reached the last, and what some of our readers may perhaps consider the most important branch of the discussion—namely, the causes of vaccine failure. This subject naturally belongs to Dr. Baron's second volume; and we shall wait with impatience for the detail of his own and of Dr. Jenner's views concerning it; but there are sufficient allusions to it in the present volume, to entitle us to bring the matter forward in this place. We must, in the first instance, refer back, for a short time, to the views originally entertained by Jenner concerning the complete efficiency of vaccine protection. Popular rumour, we have seen, attributed to cow-pox a certain degree of protecting power from the small-pox; but it was *only to a certain degree;* and the great reason why Jenner failed in convincing his early friends of the Alveston club, of the justness of his views, was, that in their judgment, the *quantum* of protection which cow-pox afforded was not sufficient. It is important to keep this in view. The following extract will explain our meaning; and those who desire further information concerning the popular opinions as to vaccine influence, may consult our author at pages 48, 49, 131, 292, and 298.

"The state of feeling of those medical men in the district where he resided, who had heard of the reported virtues of the cow-pox, and who had also occasionally met with occurrences which seemed to corroborate the popular rumour, will have told the reader the sort of difficulties that Jenner met with, and will likewise prove that it required a mind possessed of all the firmness of purpose which he enjoyed, to induce him to persevere in his pursuits. 'We have all heard (they would observe) of what you mention, and we have even seen examples which certainly do give some sort of countenance to the notion to which you allude; but we have also known cases of a perfectly different nature,—many who were reported to have had the cow-pox having subsequently caught the small-pox. The supposed prophylactic powers probably, therefore, depend upon some peculiarity in the constitution of the individual who has escaped the small-pox; and not on any efficacy of that disorder which they may have received from the cow. In short, the evidence is altogether so inconclusive and unsatisfactory, that we put no value on it, and cannot think that it will lead to any thing but uncertainty and disappointment.'"—pp. 125, 126.

From this and other passages we may learn, that there were two supposed objections to vaccine inoculation:—1. The *temporary* nature of the security afforded.—2. Its occasional want of power to saturate the system. Mr. Alsop of Calne, told Dr. Ingenhousz, in 1798, "that the casual cow-pox is sometimes not severe enough to extinguish the susceptibility to the small-pox." (p. 292.) It is certainly curious, that Dr.

Jenner should have acknowledged the full value of the popular opinions regarding the security of cow-pox, but should have neglected, or at least treated lightly, the qualifying clauses. Experience has now shown that common observation was pretty nearly as successful in the one case as in the other. But the question naturally occurs, how did Dr. Jenner meet these objections? It was in the following manner:—1st, By adopting the notion of a true and spurious cow-pox, one of which afforded full protection, and the other little or none. (p. 132.)—2d, When he afterwards learned that the true cow-pox itself could not always be depended on, he adopted the theory of deterioration of the virus, and contended that cow-pox, in its deteriorated state, may have produced a local disease, but "no such influence upon the constitution as is requisite to render the individual unsusceptible of variolous contagion." (p. 133.) With these two qualifying explanations, Dr. Jenner confidently announced the *completeness* and the *permanency* of vaccine protection. The preamble of the petition to parliament, formerly alluded to, runs thus:—"Your petitioner having discovered that a disease, known by the name of cow-pox, admits of being inoculated on the human frame, and is attended with the singularly beneficial effect of rendering, *through life*, the persons so inoculated *perfectly* secure from the infection of the small-pox, therefore," &c. The same sentiment is no less strongly expressed at p. 294, in a letter to Dr. Ingenhousz, dated in 1798. "At present, says he, I have not the most distant doubt that any person who has once felt the influence of *perfect* cow-pox matter, would ever be susceptible of the small-pox.

In process of time, however, when some of those persons vaccinated even by himself, took small-pox, a third explanatory principle was adopted, which, however, is not to be found in any of Dr. Jenner's early letters. This we would term the principle of *variolous diathesis;* in other words, the occasional imperfection in the security afforded by small-pox itself. To give a strong instance, Louis XV. died of a second attack of small-pox, at the age of sixty-four, having undergone that disease at the age of fourteen years. This event made a great sensation throughout Europe." (Baron's Life, p. 226.) Much importance has always been attached to this circumstance as a source of vaccine failure. We do not disregard it, but we think it very much overrated. Its validity proceeds upon the assumption that small-pox and cow-pox are essentially the same diseases, a position which

we have already shown to be at least questionable. That it is inapplicable upon a great scale is obvious, from the vast disproportion between the cases of recurring small-pox, and of small-pox after vaccination. Jenner was himself so sensible of this, that, subsequent to the period comprised in Dr. Baron's volume, he enlarged the theory of imperfect vaccination, and admitted, as a fourth principle, diminishing the influence of vaccination, pre-occupation of the skin, by herpetic or other eruptions, during the progress of the cow-pox. We are warranted by his own words * in stating, that he attributed very great importance to this very pathological doctrine. He calls it the grand impediment, and expresses his pleasure that continental nations now begin to estimate its influence."

Such are the views taken by Dr. Jenner, of the causes of vaccine failure. We should close the argument here, if we were not desirous to say a few words on an hypothesis which is often brought forward in common conversation as capable of explaining, in a great degree, the occurrence of small-pox after vaccination. We mean deterioration of the virus from passing through so many successive bodies; and the remedy proposed, is, of course, more frequent recurrence to the cow. This idea, we believe, was never countenanced by Dr. Jenner, and it is not generally adopted by medical men. It appears to us altogether unworthy of credit; for, first, some of those who were vaccinated by Jenner himself, in the very infancy of the practice, have since taken small-pox. The first lady of rank who was vaccinated in this country (the present Countess of Denbigh) had small-pox after vaccination, very favourably, in 1822. A son of Admiral Barclay, the steady friend and supporter of Jenner, vaccinated in 1806, to the perfect satisfaction of Dr. Jenner, had small-pox also in 1822, somewhat more severely, but not at all dangerously. 2. Those who take cow-pox naturally, or direct from the cow, are still sometimes attacked by small-pox. 3. It is contrary to the analogy of small-pox, which is as virulent now, as it was in the days of Rhazes and Avicenna. 4. We have strong grounds for suspecting, that the cow-pox, instead of deteriorating in quality by successive inoculations, actually improves; of this, at least, we are sure, that by careful selection of healthy subjects, a pock comes at length to be formed, which passes through its stages with a regularity and beauty most gratifying to witness. But we must

* See Lond. Med. and Phys. Journal, for September, 1822. vol. 48. p. 194.

draw this article to a close. We are afraid we may have already wearied our readers, and find our only hope of pardon in the great interest and paramount importance of the subject. One word, however, by way of peroration.

In the remarks which we have here ventured to offer on a momentous question, we hope not to be misunderstood. We deprecate the idea of being branded with the name of enemies of vaccination, because we have freely canvassed its merits, and in some instances exposed its weaknesses. If we thought we really incurred such a danger, we would quickly turn the tables upon our readers, and by a description of the horrors of the natural small-pox, drawn, we can hardly say, from the life, but at least, from nature, harrow up their souls, and "make each particular hair to stand on end, like quills upon the fretful porcupine." We would refer to books, and by a detail of the dreadful miseries which arose out of the general practice of small-pox inoculation, make them rather suffer patiently "their present ills, than fly to others which they know not of." If we spare our readers these painful recitals, it is upon the understanding, that we are not to be looked upon as the opposers, no, nor even as the lukewarm friends, but as the zealous and firm supporters of vaccination. What then, it may be asked, is the spirit and tendency of our argument? We will answer, in language which cannot be misunderstood. Our object has been to impress upon the public mind an old fashioned, but very important principle, which somehow or other has been strangely neglected of late,—namely, that we live in a world, in which, for truly wise but often inscrutable purposes, Providence permits that good and evil should be blended together. We shall not be able to eradicate small-pox, but we have been blessed with several means of lessening its virulence. It has been our further design to show, that the study of those means is an intricate and difficult one, and peculiarly the duty of those who follow the profession of physic.

In conclusion, we earnestly desire to instil into our readers a firm reliance upon their professional advisers, in all matters which concern the security of themselves and their families from the small pox. We have attempted to show, and, we trust, have succeeded in proving, that the subject engages the earnest attention of medical men; and we are deeply impressed with the conviction, that so long as they (with scarcely one dissentient voice) recommend to others, and in their own families practise the inoculation of the

cow-pox, so long will that practice deserve the confidence of the public.

We beg pardon of Dr. Baron for having lost sight of him so long. After what has been said, we need not occupy much time in assuring him of the high regard in which we hold his contribution to the literature of the day.

Illustrations of the Elementary Principles of the Structure of Language.

[By a Friend of the Editor *.]

The following remarks refer, primarily, to the Elementary Principles of the Structure of the Hebrew Language; but, as this is the most simple of tongues, as it is the primitive stock from which other languages have sprung, it is evident that all investigations respecting the first principles of the structure of language must be founded upon an analysis of the composition of that original fountain whence language in general is derived. And it will be found, that the elementary principles which regulate the structure of the Hebrew language, form the basis also of other languages, and furnish us with the key to etymology in general.

Vocabularies and dictionaries of the Hebrew language present a list of about two thousand primitive words which are called *roots*, from which the other words in the language are constructed. Each of these *primitives* or *roots*, is composed of *two* or *three* letters only, or, in other words, is either a *biliteral*, or a *triliteral;* and each of these is usually regarded as an arbitrary word, arbitrarily constructed, and endowed with an arbitrary import. But it appears, even on the first view of the subject, highly improbable, that so many *biliterals* and *triliterals* should be mere arbitrary combinations of letters; such simplicity of structure seems to argue the existence of some connexion between the structure of each *root*, and its applied import —a connexion between the assigned import of each word, and the symbolical or ideal import of the letters which compose it.

* This Paper contains the outline of a communication which was read before the Members of the Royal Institution, on Friday evening, the second of March, 1827.

Such was the impression which I received, when I first turned my attention to the Hebrew language; and, under the conviction that such was the fact, I endeavoured to analyze the composition of each root, and to find out the powers of the letters, so as to reduce each root to its primary elements. In conducting this analysis, I have depended solely on the results of repeated comparisons of each root with other roots, and I have succeeded in resolving the *tri*-literals (or roots of three letters) into *bi*-literals, (or roots of two letters), and in resolving the biliterals into single letters. In other words, it has been found that each triliteral is a derivative of a biliteral; and that each biliteral may be traced to a single letter, of which it is to be regarded as a derivative, and upon the symbolical or ideal import of which its ideal, and, consequently, its assigned import is dependent. By this process, all these roots may be resolved into a few simple elements, may be reduced to the expression of a few general or leading *ideas*, the extensions, or ramifications, of which form the whole imagery of this simple and original language.

We proceed, then, to the consideration of the powers of the letters.

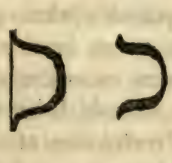
Letters, or characters traced on a surface, are to be regarded as *signs*, either of *things*, or of *sounds*. As signs of *things*, they are to be considered as exhibiting some *representation* of the *thing* which they are intended to denote, and as addressing themselves to the *eye*. But, as every letter has a certain *sound* assigned to it, by which it is expressed by the organs of speech, it becomes the *sign* also of that *sound*, and, as such, it addresses itself to the ear. So that the *sign* of a *thing* is the *sign* of a *sound* also, and it addresses itself both to the *ear* and to the *eye*.

Now it is evident, that letters, or characters, regarded as the *signs* of *things*, might be as numerous as the *things* which are to be *represented*. The *sounds*, however, by which they can, severally, be expressed by the *organs of speech* are few and simple, and as these *organs* consist of the *lips*, the *teeth*, the *tongue*, the *palate*, and the *fauces*, or *guttur*, the vocal sounds, as formed and modified by these organs, have been distinguished as *labial*, *dental*, *lingual*, *palatine*, and *guttural*. So that these *five classes of sound* embrace all the vocal sounds by which any letters can be expressed.

The Hebrew alphabet contains twenty-two letters, each of which has a certain *sound* assigned to it; but, as there are but *five* classes of *vocal sounds*, it follows that different letters must be expressed by sounds belonging to the same class of sounds; the twenty-two *sounds* of which the twenty-two *letters* are the *signs*, must be arranged under the *five classes* of sounds which have been enumerated. If, then, each letter were the *sign* of a *distinct thing*, it is evident, that two letters, which represent two distinct and dissimilar *things*, might be expressed by sounds of the *same class*. So that although, if regarded as *signs* of *things*, there might not be the slightest similarity or connexion between them, yet, as *signs* of *sounds*, they might bear so close an affinity to each other, as to be liable to be substituted for each other by those who regarded them only as *signs* of *sound*.

If each letter of the Hebrew alphabet were the *sign*, or *representation* of a distinct *thing*, the whole alphabet would present the signs of twenty-two *things* only. Such a limited number of signs of things being utterly insufficient for the purposes of language, it was necessary to connect, with the *thing represented*, some general, or leading *idea*, which bore some *resemblance to*, or had some *analogy with*, the *thing represented*. So that a letter which was the *sign* of a *thing*, was made also the sign or symbol of some *general idea* which was associated with that *thing*.

The letter כ, for instance, which is called Caph (כַּף), or *bow*, *represented*, as both its name and its present form denote, a *bent bow*:



This may be called its *representative* character.

With the representation of a *bent*, or *strung bow*, was naturally connected the idea of *curvature*, and, consequently, this letter (the *bow-letter*) was adopted as the symbol of *curvature*, or *incurvation*. This may be called its *ideal* character.

With a *bent* or *strung bow*, was also associated the idea of

restriction, the instrument being *strained* or *rigidly forced* into, and retained in the *bent*, or *curved* form, by the string*. Hence the bow-letter כ was adopted as the symbol of *restriction* also.

But the *bow* was the instrument of *smiting*, *wounding*, *piercing*, or *infixing* with arrows†; hence it was adopted also as the symbol of *smiting*, *wounding*, or *infixing* generally.

Thus the representation of a *bent bow* became the sign or symbol of *three general leading ideas*, namely—of *incurvation*, of *restriction*, and of *wounding*, *infixing*, or *smiting*.

The bow-letter כ, then, had a *representative* character, and it had also a three-fold *ideal* character. But it was expressed by a certain *sound*, of which it also became the sign, (a sound answering to that of the Greek K and Latin C), and that *sound* is referrible to the *palatine* class of sounds. The sound by which any letter is expressed may be termed its *phonic* character, and every letter which is the sign or symbol of an *idea*, or, in other words, which is endowed with an *ideal character*, may be called a *significant*.

The several *characters* of the significant כ may, then, be expressed thus:—

Representative character:		a *bent Bow*.
Ideal - -	I.	*Incurvation*.
	II.	*Restriction*.
	III.	*Smiting*.
Phonic - - -		*Palatine*.

Being thus made the symbol of three general comprehensive ideas, the *bow-letter* (which, when inverted, becomes the Roman C) was made capable of very extensive application, and it will be found that every word in the Hebrew language which is expressive of *incurvation*, of *restriction*, and of *smiting* or *cutting*, may be traced to this significant.

* Thus the noun f. קשת, which also is used to denote *a bow*, signifies *rigid*, or *contracted*, from קש, the derivative of כש, which is formed from כ. [See Table II. 2. 3. a.]

† So קשת, in Chaldee, is a verb, signifying, *to smite with a dart*.

The following table shews the various extensions or ramifications of the three general ideas, of which the bow letter is the symbol; it is constructed from a general and particular review of the various applications of the numerous words which may be traced up to this significant.

Extensions or ramifications of the three ideal characters of the bow-letter ɔ.

- I. *INCURVATION.*
  - 1. *Incircling.*
    - *a. Inclosing.* — *Incasing.* / *Collecting together.* See II. 2. *a.*
    - *b. Including.*
    - *c. Covering.*
    - *d. Embracing.*
    - *e. Going round about.*
    - *f. Binding round.*
    - *g. Forming a circle.*
    - *h. Cutting round.*
  - 2. *Binding round.*
    - *a. Arched.*
    - *b. Coved.*
    - *c. Curved.*
    - *d. Bent.*
- II. *RESTRICTION.*
  - 1. *Restraining.*
    - *a. Checking.*
    - *b. Prohibiting.*
  - 2. *Contracting.*
    - *a. Gathering together.* See I. 1. *a.*
    - *b. Coagulating.*
  - 3. *Constringing.*
    - *a. Rigidity. Firmness.*
    - *b. Binding.*
    - *c. Holding.*
  - 4. *Fixing.*
    - *a. Affixing.*
    - *b. Appointing.*
    - *c. Settling.*
- III. *SMITING.*
  - 1. *Cutting.*
    - *a. Cutting off.*
    - *b. Cleaving.*
  - 2. *Hitting.*
    - *c. Striking in a sharp instrument.* — *Staking.* / *Biting.*

We now come to the consideration of the biliterals which are constructed from this significant.

It is evident that, if this significant stood alone, it might be the symbol of either, or of all the *ideas* which are associated with it. In order to make it available for the purpose of expressing each ideal character distinctly, it was necessary to couple it with different signs or letters. Nor was it necessary that such adjunct signs or letters should exert any symbolical character; for if, for example, we possessed three letters which were not endowed with any ideal character, we might affix or prefix one of these to our significant, when we wished to employ it with a reference to its first ideal character: we might use another of these in conjunction with the same significant when we intended to use it with a reference to its *second* ideal character; and the third might be connected with that significant when it was used with a reference to its *third* ideal character. We might thus construct three biliterals from this significant, in each of which a different ideal character might be exerted, the limitation of each biliteral to the expression of one particular idea (to the exclusion of the two other ideas) being altogether arbitrary, although, when once made, it would ever after remain fixed by the authority of usage. Auxiliary letters, such as we have just supposed, for the sake of illustration, to exist, actually do exist in the Hebrew alphabet; letters which, although they exert no symbolical character, are nevertheless instrumental in enabling the same significant to exert, under different combinations, its several ideal characters, or the several modifications of which its single ideal character is susceptible, separately and distinctly. Such letters are termed *formatives*.

Some of the letters of the Hebrew alphabet act always the part of *formatives;* others always act the part of *significants;* and there are some letters which, under certain circumstances, are merely *formative*, while, under other circumstances, they act the part of *significants*.

Let us revert to the significant כ, or the *bow*-letter.

If we affix to this significant the letter ל, (which here acts the part of a formative) we construct the biliteral כל, or CL, which is expressive of *in-CLosing, in-CLuding, keeping CLose*, or

restraining. Thus, in this biliteral, the bow-letter exerts its ideal characters I and II. [See Table I. 1. a—e. II. 1. 2.]

Hence we have the Greek ΚΛειω, to *shut* or *CLose*, (in Latin *CLaudo*, in Welch *KLoi)*; ΧωΛος, and ΚυΛλος, *CLaudus*; ΚωΛυω, to *restrain*; ΚοΛλη, *GLue*; ΚαΛυξ, a *CaLyx*, cup, or thing in which something is *kept, or inCLuded*; ΚαΛως, a *cable*; ΚοιΛος, *cavus*; and the Latin *CauLa*, *CeLla*, *CœLum*, *CuLeus*, *CeLo*, *CoLo*, *CuLo*, &c. And as the biliteral כל is also indicative of *bending round*, (Table I. 2,) it gives origin to the Greek ΚΛινω, whence the Latin in-CLino, and the English *in-CLine*.

If we affix to the bow-letter the letter ש, (which here acts the part of a formative) we construct the biliteral שכ (CS) which is expressive of *in-CLosing*, *in-CLuding*, *in-CaSing*, *covering*, or *contracting together*. [See Table I. 1. a. b. II. 2.] Thus, in this biliteral, the significant כ exerts its ideal characters I. and II.

Hence we have the Greek ΚαΖω, *orno*; ΚοΣμος, *ordo*; ΚιΣτη, CiSta, CheSt; ΚιΣσος, *ivy*; the Latin CaSsis, something which *in-Cases*, or *in-CLoses*, a *net*, *trap*, *helmet*, or *cap*; CaSa, a *HouSe*; the English *CaSe*, *CaSh*, &c.

If we affix to the bow-letter the letter פ, (which here is merely constructive, it acting the part of a formative) we construct the biliteral פכ (CP or CPh), which is expressive of *incurvation*, (and the various applications and modifications of that idea; see Table I. 1. 2.) Thus in this biliteral, the significant כ exerts its ideal character I.

Hence we have the Greek ΚυΦος, *curvus*; ΚαμΠτω, *to bend*; ΚηΠος, an *inclosed garden*; the Latin *CaPio*, to *KeeP*, *hold*, *compass*, or *retain* [See Table I. 1, II. 3, c.], *HaBeo* also having the same assigned and ideal import; *CoPia*, a quantity *collected together*. [See Table I. 1. a. II. 2. 3.] *CuPio*, *to in-CLine towards*, *(mentally,)* or to *grasp at*, *cherish*, or *embrace (mentally)*, *HoPe* having the latter ideal import, as *ChuSe* the derivative of כש also has; *CaVus*; and the English *CaP*, *CaPe*, *CooP*, *CoPe*, *CoVe*, *CaVe*, and a long list of other words.

With the letter ר affixed, (which here exerts little or no ideal import, it being constructive only, or formative,) the bow-letter

forms the biliteral כר (CR) which denotes: *CiRcularity*, or *in-CuRvation*. So that in this biliteral the significant כ exerts its ideal character, I.

Hence we have the Greek XoPos, ΓυPos, and KιPxos; as also XειP, which in Hebrew is כף; the Latin *CiRca*, and *CiRcum*, and the English *CiRcle* and *CuRve*.

This biliteral (כר) also denotes, *cutting round*, or *amputating;* (so the Greek ΠειPι, signifies *CiRcum*, and ΠPιω, to *Cut;* and *CaRve* as well as *CuRve* is derived from כר). See Table I. 1. h.] Hence we have the Greek KειPω, *abscindo*.

With the formative letter ת affixed, the bow-letter forms the biliteral כת (CT), which is expressive of *smiting*, *CuTting*, or *HiTting*. So that in this biliteral, the significant כ exerts its ideal character III. [See Table.]

Hence we have the obsolete Greek form KoTτεω, (and, from a figurative application of the idea, KoTεω;) the Latin *CœDo* and *CuDo*, and the English *CuT* and *HiT*.

With the formative letter ש prefixed, the bow-letter forms the biliteral שך (SC), which is expressive both of *restricting* (II.), and of *infixing*, or *piercing* with a *sharp* or *cutting instrument*, (III. 1, a, b, c.); so that in this biliteral, the significant כ exerts its ideal characters II. and III. [See Table].

Hence we have (as used in the former of these imports), the English *CheCk;* and (as used in the latter of these imports) the Latin *SeCo*.

These are the principal biliteral derivatives of the bow-letter. There are some others which may be briefly enumerated.

With the aspirate ה affixed, (which is here merely formative) the bow-letter forms the biliteral כה, which is expressive of *rigidity*, *firmness*, [II. 3. a. see Table.] So that, in this biliteral, the significant כ exerts its ideal character II.

With the formative נ affixed, the bow-letter forms the biliteral כן (CN), which is expressive of *fixing*, *establishing*, *making firm*, [II. 4. see Table]. So that, in this biliteral, the significant כ exerts its ideal character II.

With the formative ה, the bow-letter forms the biliteral כה (CE), which is expressive of *restriction*. (It is also a particle of *restriction*, as כ is also when it occurs alone as a prefix to a word, or when it is followed by the formative י as כי (CI).

So that, as a *prefix*, or when followed by the soft aspirate or vowel ה, or by the vowel י, the significant כ exerts its ideal character II.

With the formative א affixed, the bow-letter forms the biliteral כא (CA), which is expressive of *smiting*. So that, in this biliteral, the significant כ exerts its ideal character III.

With the formative ו affixed, the bow-letter forms the biliteral כו (CU or CV), which is used, in a figurative sense, to denote the *pungent* action, or effect of heat. And with the formative מ affixed, as כם (CM), the bow-letter is similarly applied. So the English *HoT* and *HeaT* have the same primary import, and the same common origin as *HiT* and *CuT*, they being endowed with the import of כת (CT), the derivative of כ, from which significant is also formed the Greek Καιω, *uro*. So that in the biliterals כו and כם, the significant כ exerts its ideal character III.

We have thus taken a hasty view of the mode in which biliterals are constructed from the significant under consideration. In some of these, the significant exerts one only of its ideal characters; in others, two of these.

Thus it exerts its ideal characters I. and II. in the biliterals כל, כש, כף, כר; its ideal character II. in the biliterals כה, כי, כח, כן, (כן also, in some instances, conveys the ideal character I.); it exerts its ideal characters II. and III. in the biliteral שך; while in the biliterals כא, כו, כם, and כת, it exerts its ideal character, III.

These, then, are the *Primary* biliteral derivatives of the significant כ; we now come to the consideration of the *secondary* biliteral derivatives of the same significant.

We have seen that the bow-letter כ is not only the symbol or sign of *ideas*, but that it is also the sign of a *sound*, which sound belongs, as has been already stated, to the *palatine* class of sounds. [See the *phonic* character of כ.] Now there is a most important rule, which is extensively prevalent in the Hebrew language (as we at present find it) and in other languages, which may be thus stated: *letters of the same organ of speech* (i. e. letters whose *phonic characters* belong to the *same class* of vocal sounds) *are mutable with*, or *liable to be substituted*, or *ex-*

changed for, each other. Hence it happens, that *a letter*, or *the letters*, of a *primary biliteral* is, or are, exchanged for some other letter, or letters, of similar phonic character: and, in this way, *secondary biliterals* are formed, which are endowed with the import or imports of the *primary biliterals* from which they are respectively derived. We will illustrate this mode of constructing *secondary biliterals*.

We have seen that the biliteral כת is formed, by affixing the formative ת (T), to the significant כ (C), and that, in this biliteral, this significant exerts its ideal character III.; the biliteral כת, denoting CuTting or HiTting. This, then, is an example of a *primary biliteral*. But, examining this biliteral with a reference to the *phonic* characters of its component letters, we find that it consists of a palatine (כ) followed by a dental (ת.) If, then, taking this primary biliteral, we substitute for its first, or *palatine* letter, some other *palatine* letter; or, if we substitute for its final, or *dental* letter, some other dental letter; we may still construct a biliteral, which is endowed with all the import of כת, although the first letter, or the last letter of this latter biliteral have been exchanged for another letter, or although both its letters have been exchanged for two other letters, the first of such substituted letters being a *palatine*, the second a *dental*.

The other palatines are ק, whose *phonic character* closely resembles that of כ, it being similar to that of the Latin K or Q; ג answering to the Latin G; and the harsh aspirate ח, answering to the Greek X, or the Latin Ch.

Thus from the *primary* biliteral כת, we form (by substituting the palatine ג or ח, for the first or *palatine* letter כ,) the *secondary* biliterals גת (GT) and חת (HT or ChT), which are endowed with the import of כת. So, by substituting for the final or *dental* letter of כת some other dental letter, and by also exchanging the first, or palatine letter, for some other palatine, we also procure, from this primary biliteral, the secondary biliterals קט (KT, KTh, QT, or QTh), קץ (KZ, or KTs), חץ (HZ or ChTs), חז (HZ or ChDs), גז (GZ or GDs), חד (ChD or HD), all of which are endowed with the import of the primary biliteral כת (CT).

In like manner, if we take the *primary* biliteral כש (CS), and if we substitute for its final letter ש, the cognate (or *similarly-sounded)* letter ס (S or Sh), we construct the *secondary* biliteral כס, which has the same import. So if, taking the same primary biliteral, we substitute for its first, or palatine letter כ, some other palatine, as ק or ג, we still get a biliteral endowed with the same import; thus קש, and גש, possess the import of כש. So by exchanging the first letter of this primary biliteral for the palatine aspirate ח, and the final letter for its cognate ס, we construct the biliteral חס, which is endowed with the full import of כש. From the *primary* biliteral כש, then, are formed the secondary biliterals כס, קש, גש, and חס.

So from the *primary biliteral* כף (CP), we construct (by substitution of some other palatine for the palatine כ) the *secondary* biliterals קף, חף, גף; and (by substitution also of the labial ב for its cognate פ) the secondary biliterals קב, חב, גב; each of these *secondary biliterals* being dependent for its import upon the primary biliteral כף.

Secondary biliterals are formed from other primary biliteral derivatives of כ in a similar manner, but the enumeration of these would extend this paper to too great a length.

Triliterals are formed from *primary* and *secondary* biliterals in three ways. First, by *affixing* to the biliteral some formative letter, (thus כלא and כלה and כלם are formed from כל; קטב, קצב, and חצב, from קט, קץ, חץ; קשר from קש;) or, by repeating the final letter (as כלל from כל; כפף from כף.) Secondly, by *inserting* a formative letter (as כהל from כל, כהן from כן, כנש from כש.) Thirdly, by *prefixing* a formative (as יכל, נכל, שכל, from כל). Such are the simple modes in which triliterals are constructed from biliterals.

The observations which have been made respecting the construction of *secondary* biliterals from *primary* biliterals, by substitution of letter or letters of similar phonic character, apply also to triliterals. Thus a triliteral which has been constructed from any biliteral (whether primary or secondary) may have one or more of its letters exchanged for some other letter or letters of similar phonic character; and, in this way (as we observed when treating of the construction of secondary biliterals) is formed a

secondary triliteral, or a second series of triliterals, and each such secondary triliteral is endowed with the import of its parent. Thus from the triliteral קשר is formed (by exchanging its first, or its second letter, or both these, for a letter or letters of similar phonic character) a long string of secondary triliterals, all and each of which derive their import as well as their origin from that parent triliteral, which is the derivative of קש, a *secondary biliteral*, (whose import is communicated to its derivative קשר), which derives its origin and its import from the *primary biliteral* כש, which is formed from the significant כ, whose ideal characters I. and II. are exerted in it.

These, then, are the modes in which triliterals are constructed.

In these several ways, then, the bow-letter כ communicates one or more of its ideal characters to biliterals and triliterals; and, in a similar manner, other significants impart their import to their derivative biliterals and triliterals. The bow-letter is, however, the most important of all the significants, inasmuch as we may trace up to it a greater number of what are called *roots*, than can be traced to any other significant. The phonic character of this significant (כ) is, as we have stated, *palatine*, (it having the *sound* of the Latin C or K, and it being, hence, mutable with the *palatines* ק, ג, and ח, which answer to Q or K, G, harsh H or Ch.) But the harsh sound of כ seems to have been sometimes exchanged for a softer sound, approaching to that of a *sibilant*.

So the letter C (which is formed by inverting the bow-letter) is both a *palatine* and a *sibilant*, ("aut *cornix* aut *serpens*,") in English as well as in Latin (a *palatine* in *cup*, a *sibilant* in *city*), and in the older Greek alphabet, the sibilant S was denoted by the character C.

For we find, not only that there is a set of *secondary* biliterals formed from the *primary* biliteral derivatives of כ, by exchanging the significant כ for some other palatine (as when קף, גף, חף, are formed from כף), but that there is also, in some instances, a *second series of secondary biliterals*, which is also formed from a primary bililiteral derivative of this bow-letter, but which is constructed by the substitution of a *sibilant* or *dental* letter for the bow-letter. Such we find to be

the case with regard to the primary biliterals כף (CP), and כר (CR).

Thus, from כף, we not only get the biliterals קף, נף, חף, קב, נב, חב but also the biliterals סף, שב, סב, טף, תף, (טב, תב,) צף, (צב), each of these being primarily dependent for its import on the primary biliteral כף, to which the significant כ imparts its ideal characters I. and II.

So כר (CR), not only communicates its import to its derivative חר (and, in several instances, to קר and to נר), but it imparts it also to the biliterals סר, טר, דר, צר, and, in many instances, to זר, שר, and תר.

Thus a *second set* of *secondary biliterals* is derived from the significant כ, and from each of these (as from the other biliterals, both primary or secondary) are formed various triliterals, which are constructed in the manner already pointed out, namely, by adding a formative letter, either as an *affix*, or as an *epenthetic*, or as a *prefix*.

In these several ways, then, are triliterals, (both *primary* and *secondary*) constructed from biliterals, which biliterals are either *primary* or *secondary* (there being, as we have seen, both a *first* and a *second class* of secondary biliterals in some instances); and thus, from about eight or nine *primary biliterals*, which owe all their import to one single significant (the *bow-letter* כ), may be derived and constructed above five hundred of those words which are set down in the dictionaries and vocabularies of the Hebrew language as arbitrary *roots*. So that from this apparently simple symbol, from the extension of the three general ideas—*incurvation*, *restriction*, *smiting* or *infixing*, is constructed more than a fourth part of the whole language.

In a similar manner, as we have before observed, do other significants communicate their imports to the biliterals and triliterals which are derived from them. But the great length to which this paper has already been extended, obliges us to reserve, for some other communication, all remarks respecting them. I may briefly observe, that the same simplicity of structure which has been traced out in the formation of biliteral and triliteral derivatives of the bow-letter, is also seen in the construction of

those biliterals and triliterals which derive their origin and their import from other significants. And I may remark, that the analysis, of which the preceding pages afford a cursory specimen, is not offered to the notice of the public as an example of ingenuity, or of fanciful speculation, but as something which, I think, approaches very nearly to truth and correctness. Those who can form an estimate of the labour and difficulty which attend a general and particular analysis of the whole of a primitive language, and especially an analysis, in the conduct of which no assistance has been derived from any other source than that of repeated comparisons of each word with other words, will not too severely scan those inaccuracies which further emendation may correct. A specimen of the analysis (comprising the derivatives of the four palatine significants, כ, ק, ג, ח,) is, in manuscript, in the hands of the Editor of this Journal, and it may be examined by any one who feels interested on the subject of language in general, or of the Hebrew language in particular. With regard to the latter language, I may state, that the analysis to which this paper refers, exhibits the whole of this language under a simple arrangement, which will enable any one to acquire a thorough knowledge of it in a very short time with little or no labour, and will furnish an easy guide through the labyrinths of etymology in general. There is nothing in it which can tend, in the slightest degree, to obscure or mysticise the contents of the sacred volume; on the contrary, it simplifies greatly the study of the Scriptures in that original language in which they ought to be studied, while it tends to confirm most strongly the faith of the believer, and to illustrate the divine origin of our holy religion.

An Account of the Arcs perpendicular to the Meridian, which are now measuring on the Continent of Europe.

It has been known that, for several years past, France, in conjunction with Austria and the governments of Northern Italy, has been engaged in the measurement of two arcs perpendicular to the meridian, commencing on the shores of the Atlantic. Of these, the southernmost, in the latitude of forty-five degrees, the mean latitude between the equator and the pole, crosses France, Italy, and the Austrian dominions east of the Adriatic, until it impinges on the Turkish frontier; the other, in the parallel nearly of Paris and Vienna, crosses the territories of France and Austria, until it reaches the Russian frontier.

The geodesical measurements of these arcs originated in the early part of the present century, and have been since unremittingly prosecuted, excepting during the years 1813 and 1814, when the great political events of that period occasioned their suspension: on the restoration of tranquillity in Europe they were resumed with increased activity, so as to be now far advanced towards their completion. Since that period also, the necessary operations have been commenced, and carried on in concert by the respective nations, wherever such concert was required, for the determination of the celestial amplitudes, corresponding to the portions of the parallels which have been geodesically measured. Within the last few months both series of operations, the astronomical and the geodesical, have been completed on those portions of the arcs which are comprised within the French territory; and the details, necessarily very voluminous, are in preparation for the press. Meanwhile, public curiosity has been greatly interested to learn the tendency at least of the evidence, which this method of research on the figure of the earth, now for the first time employed, may afford in regard to the amount of the compression; and in consequence it has been judged proper, by the French authorities, to anticipate the final publication, and to communicate, in the present stage, such particulars concerning the portions completed, on the authority of

the persons who have conducted the observations, and who are charged with the subsequent calculations, as they deem themselves to have satisfactorily deduced from the observed elements, and which will be ultimately justified by the publication of the elements themselves. For this purpose, reports have been drawn up by the superior officers of the Corps des Ingénieurs-géographes, charged with the operations within the territory of France on both the parallels, and have been published by order of the French government, in the eighth volume of the Mémorial Topographique et Militaire redigé au Dépôt Général de la Guerre.

A memoir has also been printed on the measure of the arc of the parallel of forty-five degrees, which had been previously read to the Academy of Sciences, by Colonel Brousseaud, of the Ingénieurs-géographes, and M. Nicollet, membre du Bureau des Longitudes, who had been employed in the determination of the celestial arc of that parallel comprised within the limits of France. By the courtesy of the French government, copies of the eighth volume of the Mémorial Topographique et Militaire have been received by individuals in this country, who have been engaged in operations, or in investigations having the same ultimate purpose; and by the personal kindness and attention of M. Nicollet, copies of his highly interesting memoir have also been received. We avail ourselves of the occasion to make more extensively known in England the progress of undertakings, in the accomplishment of which several of the principal states of the continent are united; as well as the decided tendency of the results which have been hitherto obtained, to indicate, in accordance with Pendulum experiments, and the general deduction from the best arcs of the meridian that have been measured, a much greater compression than $\frac{1}{306}$, in which, until the last three or four years, the general belief appeared to be almost fully established.

We shall commence with the arc of the parallel of forty-five degrees. The western extremity of this arc is determined by its intersection with the meridian of Marennes, a town of France, on the shores of the Atlantic, situated a few miles north of the discharge of the waters of the Garonne. Traversing France, Piedmont, and Northern Italy, and connecting

itself in passing with the fixed observatories of Geneva and Milan, its eastern limit has already reached Fiume, on the shores of the Adriatic; and, by the operations with which the Austrian government has charged itself, and which are in progress, it is designed to be still further extended to Orsowa, a town of Transylvania, on the frontiers of Turkey. The portion of the parallel comprised between the meridians of Marennes and Fiume, is between fifteen and sixteen degrees: between Marennes and Orsowa, twenty-three degrees and a half. The geodesical operations applied to the measurement of this arc were commenced in Italy, Piedmont, and Savoy, in 1802, having for their object the formation of a topographical map of those countries, corresponding to that of France by Cassini; the project of measuring so extensive an arc perpendicular to the meridian, not having been then formed. Their execution was confided to the officers of the French corps des Ingénieurs-géographes, who by the year 1814 had covered the greater part of Italy, Piedmont, and Istria, with a triangulation, resting on a base of 9998 mètres, measured near the course of the Ticino, between Milan and the Lake Maggiore; and had thereby connected Fiume on the Adriatic, with Rivoli between Milan and the Alps. In 1811, the project of extending the triangulation across France to the Atlantic, and making it subservient, in conjunction with a determination of the corresponding celestial arc, to the deduction of the figure of the earth, was discussed at the instance of M. de Laplace, and adopted by the French government. Accordingly, in 1811, 1812, and 1813, a suite of triangles detaching itself from the great chain of the French meridian, in the parallel of forty-five degrees, was carried to the frontiers of France and Savoy; and in 1818, 1819, and 1821, similar operations connected the chain of the French meridian with the shores of the Atlantic. Finally, in consequence of a communication from M. de Laplace to the academy at Turin in 1820, the Piedmontese government appointed a commission, composed of astronomers and officers of the Austrian and Sardinian état-major, who in 1821 and 1822 connected the operations in France and Italy by a triangulation carried across the Alps: thus uniting Marennes on the Atlantic with

Fiume on the Adriatic, by an uninterrupted series of one hundred and six triangles, of which ninety were executed by the French officers, and sixteen by the Austro-Sardinian commission. The excellence of the instruments that are now employed in geodesical operations, and the great experience that the French officers in particular have had in them, might be a sufficient warrant that the terrestrial measurement has been executed with the requisite exactness. MM. Brousseaud and Nicollet have, however, placed this beyond question, by employing as bases of verification the sides of the triangles of the great chain of the meridian, which are common also to the triangulation of the parallel resting on the base near Milan *. Contemplating the prolongation of the arc from Fiume to Orsowa, it has been considered preferable to calculate from the triangles the length of the arc of the parallel of 45° 43′ 12″, instead of that of forty-five degrees exactly, as by that means a portion of the Turkish territory between the thirteenth and seventeenth degrees of longitude east of Paris is avoided: the parallel of 45° 43′ 12″ is also that which intersects the greatest number of triangles.

The operations for the measurement of the celestial arc were not commenced until 1821; the method adopted for this purpose was, by the explosion of small quantities of powder, on summits of such elevation, that the flash might be simultaneously seen at two stations, including between them considerable differences of longitude; when, the instant of the apparition being registered at each station in its own sidereal time accurately known, the comparison of the registers might show the difference of time, or, in other words, the celestial arc, included between the two stations. That this might be accomplished with the greater certainty as well as exactness, several explosions were made in the same night, at nearly equal and appointed intervals of time, and the signals were also repeated on several successive nights. By this method of proceeding, the total arc, the amplitude of which it is desired to measure, becomes divided into several portions, each of which is suc-

* Since the above was written, we have learnt that a base of above 14,000 metres was measured in the last summer by Colonel Brousseaud near Bourdeaux, and is to be corrected in the next summer by a chain of triangles with the western extremity of the arc at Marennes, as an additional verification.

cessively and independently determined; when the sum of the several portions is the amplitude due to the total arc. The disadvantage under which a result so obtained labours is, that it is necessarily chargeable with the accumulated sum of the errors, which may have occurred in the determination of the time, at each extremity of each of the portions into which the arc is divided: in which accumulation, however, the errors that have contrary signs will have mutually destroyed one another, proportionally to their respective amounts. As the same method was adopted in all the portions into which the arc between Marennes and Fiume was divided, differing only in the heights and in the distances apart of the stations, and occasionally in the number of signals employed, we shall limit ourselves to a notice of the stations, and of the periods at which the several determinations were accomplished.

By means of signals made at La Ferlanderie in October, 1823, the difference between the meridians of the steeple of Marennes, and a temporary observatory established at the mill of St. Preuil, was ascertained by M. Nicollet, and Colonel Brousseaud, as follows:—

October 6th, mean of 10 signals	3′ 48″,831
15th, 10 signals	3′ 48″,969
16th, 10 signals	3′ 49″,207
17th, 16 signals	3′ 49″,030
Mean of the 4 series, 46 signals	3′ 49″,012
Reduction to the steeple of Marennes	— 00″,022
Difference of meridians between the Observatory of St. Preuil and the steeple of Marennes. . .	3′ 48″,99

In the month preceding, namely, in September, 1823, the difference between the meridians of the observatory at St. Preuil and a temporary observatory established at La Jonchère, was ascertained by the same observers, by signals made on the Puy Cogneux. In this instance, from accidental circumstances, the signals were visible at both stations on a single night only, that of the 10th of September*; and the arc between

* In selecting the summit of the Puy Cogneux as a station for the signals, Colonel Brousseaud had assured himself, by personal observation from the summit of the mountain, that the mill of St. Preuil was visible from it, and had concluded, consequently, that without doubt the

them is consequently determined by the mean of not more than ten comparisons: but as all the circumstances attendant on the observations of that night were very favourable, and as the extreme discordance of the particular results amounts only to 0″,33, the determination may deserve to be received with the satisfaction which is expressed respecting it by the observers themselves. The difference of meridians thus deduced is 6′ 28″,341, which, diminished by 5″,247, the reduction of the observatory at La Jonchère to the neighbouring trigonometrical station on the Puy de Sauvagnac, makes the difference of longitude between the observatory of St. Preuil and the station at Sauvagnac, 6′ 23″,094.

In August, 1823, the same observers determined the difference of longitude between the observatory of La Jonchère and a temporary observatory at the village of Solignat, by signals made, on the 19th and 22d, on the summit of the Mont d'Or, ten signals on each night: the results were as follow:—

August 19, mean of 10 signals	6′ 49″,808
22, 10 signals	6′ 50″,171
Mean of 20 signals	6′ 49″,9895
Reduction to the Trigonometrical Station on the Puy d'Isson, adjoining the observatory of Solignat .	− 3′,846
Reduction to the Trigonometrical Station of Sauvagnac	+ 5′,247
Difference between the meridians of the Trigonometrical Station of Isson and Sauvagnac . .	6′ 51″,391

On the 6th and 7th of September, 1822, signals made on the mountain Pierre-sur-Autre, and observed at Solignat by MM. Brousseaud and Nicollet, and at a temporary observatory on Mont Colombier by M. Carlini, one of the members of the Austro-Sardinian Commission, gave the following results: six

summit would be reciprocally visible without difficulty from the mill. It appeared, however, subsequently, that this was very rarely the case, and only in certain states of the atmosphere; as, during the forty-eight days which M. Nicollet remained at St. Preuil, the Puy Cogneux was only once seen, and that for an instant, at six in the morning, the sky being slightly clouded. On the night on which the signals were observed, the sky was also clouded, and the horizon appeared by no means favourable; nevertheless the signals were very distinctly visible, although made with only a quarter of a pound of powder, being half the quantity that had been tried before.

signals were made on each night, but five only were seen from both stations on the night of the 7th :—

Sept. 6	6 signals	10′ 18″,90
Sept. 7	5 signals	10′ 18″,81
Mean of the two series, 11 signals		10′ 18″,86
Reduction to the station of Isson		+ 3″,85
Difference of longitude between the station at Isson and the observatory on Mont Colombier		10′ 22″,71

At the same period that the difference of longitude was thus ascertained between Isson and Mont Colombier, signals made on Mont Colombier, and having the time of their apparition noted by M. Carlini, were also observed at Geneva by MM. Pictet and Gautier, giving the difference between the meridians of Mont Colombier and the observatory of Geneva: whilst other signals made on Mont Tabor, and observed by M. Carlini on Mont Colombier, and by M. Plana at a temporary observatory at the Hospice on Mont Cenis, gave the difference between the meridians of the temporary observatories of Mont Colombier and Mont Cenis, at which those gentlemen were respectively established. The following are the results of these several observations :—

Between Mont Colombier and Geneva.		
Sept. 4	6 signals	1′ 35″,27
Sept. 5	6 signals	1′ 35″,60
Sept. 6	6 signals	1′ 35″,10
Sept. 7	6 signals	1′ 35″,12
Between Mont Colombier and Mont Cenis.		
Sept. 5	6 signals	4′ 43″,96
Sept. 7	6 signals	4′ 44″,28
Sept. 11	6 signals	4′ 43″,01
Mean of three series, 18 signals		4′ 43″,75

The unfavourable circumstances of the weather having caused a more than ordinary uncertainty to attend the determination of the time at the observatory on Mont Colombier, elevated 1434 metres above the sea, MM. Brousseaud and Nicollet have deemed it preferable to substitute Geneva as the eastern extremity of the arc, of which the western is the trigonometrical station at Isson, and to consider Mont Colombier as a station of transmission only.

The observations of the 6th and 7th Sept. had made the difference of longitude between Isson and Mont Colombier 10′ 22″,71; the signals given on Mont Colombier on the same nights as the observations between Isson and Mont Colombier, and observed simultaneously there and at Geneva, made the difference of longitude between those two places 1′ 35″,11. The sum, 10′ 22″,71 + 1′ 35″11 = 11′ 57″,82, will therefore be the difference of longitude between Isson and Geneva, independently of any error which may have existed in the comparison of the clock at Mont Colombier with sidereal time. There then remains to be elicited from the preceding observations the arc between Geneva and Mont Cenis, in which Mont Colombier serves again as a station of transmission; and taking, as before, the observations of those nights only when the communication with Mont Colombier was effected both from Geneva and from Mont Cenis; and when, by such means, the sidereal time at Geneva became immediately compared with the sidereal time at Mont Cenis, we have, by the observations of the 5th and 7th of September, between Geneva and Mont Colombier, 1′ 35″,36, and between Mont Colombier and Mont Cenis, 4′ 44″,12, whence the difference of longitude between the observatories of Geneva and Mont Cenis appears 3′ 08″,76.

In September, 1821, MM. Plana and Carlini ascertained the amplitude of the arc between the Hospice on Mont Cenis and the observatory at Milan, by means of thirty signals made on the Roche Melon, of which ten were on the night of the 1st, ten on that of the 2d, and ten on that of the 3d September; the results were as follow:—

Sept. 1	10 signals	9′ 00″,24
Sept. 2	10 signals	9′ 00″,28
Sept. 3	10 signals	9′ 00″,08
Mean of	30 signals	9′ 00″,2
Reduction to the Dome of Milan		+ 00″,606
Difference of longitude between the observatory on Mont Cenis and the Dome of Milan		9′ 00″,806

For reasons that are not given, but probably on account of the small extent of the arc between Geneva and Mont Cenis, the determination of its amplitude, accomplished, as we have

seen, in 1822, is united in M. Nicollet's Memoir to the amplitude between Mont Cenis and Milan, ascertained in the following year,—making but one arc between the observatory at Geneva and the Dome of Milan,—the amplitude of which is $3' 08'',76 + 9' 00'',81 = 12' 09'',57$.

In August, 1824, by direction of the Austrian government, the difference of longitude between the observatories of Milan and Padua was determined as follows by signals made on the Monte Baldo:—

August 23 . . .	10 signals . . .	10′ 43″,72
August 25 . . .	10 signals . . .	10′ 43″,11
August 26 . . .	10 signals . . .	10′ 43″,08
August 27 . . .	10 signals . . .	10′ 43″,20
Mean of four series	40 signals . . .	10′ 43″,277
Reduction to the Dome at Milan	. . .	− 0″,606
Reduction to the steeple of Sainte Justine at Padua		+ 2″,712
Difference of longitude between the Dome of Milan and the steeple of Sainte Justine at Padua	.	10′ 45″,383

There remains but a single portion more, namely, that between Padua and Fiume, to complete the entire amplitude between Marennes and Fiume; this portion has been measured by signals on Monte Baldo, visible at the same time at Milan, Padua, and Fiume; but the results are not yet reported.

The rate of the clock, and its difference on sidereal time, was ascertained at each of the temporary observatories, by zenith distances of the principal stars, east and west of the meridian, never under 30° or above 60° of altitude. Several stars were observed on the same night whenever the weather permitted, and the clocks were always established several days before the signals were commenced. As far as was possible, the apparition of the signals was noted by the clock itself; but when not visible from the spot where the observatory, was placed, two chronometers were always employed, which were compared with the clock immediately before and immediately after the signals.

We have been thus particular in relating in detail the proceedings by which the celestial amplitude was ascertained, for two reasons; first, because it is the first time that the attempt

has been made to determine the difference of longitude between the extreme stations on a line of such considerable extent, by the method of *instantaneous signals;* and, second, because the value of the ultimate deduction, as to the figure of the earth, is in fact chiefly dependent on the degree of exactitude which has been practically attained in the determination of the celestial arc, inasmuch as errors in that determination of such small amount as it would seem almost impossible altogether to avoid, far outweigh, in their consequences, the utmost error, which, in the present advanced state of geodesical measurement, could be possibly apprehended as likely to arise from that source. In the parallel of 45° a single second of time is equivalent to about 324 metres; and, of course, in an arc of 13°, which rather exceeds that comprised between Marennes and Padua, an error of 1″ in the celestial arc (could such an error be supposed to exist as the sum of all the errors of the separate determinations into which the arc has been divided) will equal 25 metres in the length of the mean degree of the parallel; the influence of which on the fraction expressing the ellipticity, deduced from the comparison of the degree of the parallel with the degree of the meridian, will amount to between 22 and 23 in the denominator of the fraction. We have therefore considered it the preferable course to enable our readers, as far as it is in our power, to form their own estimate of the measure of accuracy which has been attained.

We recapitulate, in order to present in one view, the amplitudes of the several portions forming the total arc between Marennes and Padua :—

1st Arc	Marennes and St. Preuil	.	3′ 48″,990
2d Arc	St. Preuil and Sauvagnac	.	6′ 23″,094
3d Arc	Sauvagnac and Isson	.	6′ 51″,391
4th Arc	Isson and Geneva	.	11′ 57″,820
5th Arc	Geneva and Milan	.	12′ 09″,570
6th Arc	Milan and Padua	.	10′ 45″,383
Total	Marennes and Padua	.	51′ 56″,248

The following are the geodesical measures, expressed in metres, comprised on the parallel of 45° 43′ 12″ between the meridians of the stations which form the limits of the several

arcs; with the corresponding length of a degree of the parallel, as separately deduced from each of the arcs :—

1st Arc	.	74407,385	.	.	77984,95
2d Arc	.	124182,225	.	.	77797,44
3d Arc	.	133345,511	.	.	77792,00
4th Arc	.	233087,384	.	.	77931,76
5th Arc	.	236717,395	.	.	77870,77
6th Arc	.	209256,276	.	.	77807,32
Total	.	1010996,176			

In order to obtain from these results the most probable value of the degree of the parallel, MM. Brousseaud and Nicollet have followed a method recommended by Lieutenant-Colonel Puissant, of the Corps of Ingénieurs-géographes, in a memoir on the determination of the figure of the earth, inserted in the eighth volume of the Mémorial Topographique et Militaire. This method consists in making as many equations of condition as there are partial arcs, of the form :—

$$\frac{b}{x} - \frac{T}{240} = \frac{e}{240}$$

wherein b is the arc in metres, T the observed celestial amplitude in seconds of time, e the probable error of that determination, and x the most probable value of the degree ; and these equations being treated by the method of least squares, the value of x is obtained *.

By this process the most probable value of the degree of the parallel deduced from the six preceding results is 77865,75 differing 3.15 mèt. from 77862,60, the value deduced from 1010996,176, the total arc in metres divided by 59′, 56″,948, the sum of the several celestial amplitudes ; and 1,71 mèt. from 77864,04 mèt., the value obtained by taking an arithmetical

* Or substituting for x, $\beta + x$, in which a value is taken for β, differing not more than a few units from the most probable value of the degree, and x a quantity so small that its square may be neglected ; the equations of condition may then be made of the following more convenient form :—

$$\frac{240.\,b.}{\beta} - T. - \frac{240.\,b.}{\beta^2}\,x = e\,;$$

from whence the value of x being ascertained, and added to that assumed for β, the most probable mean value of the degree is obtained.

mean of the six partial results; differences which, in respect to the ultimate employment of the number itself in the deduction of the Ellipticity, may be regarded as altogether inconsiderable.

Applying, then, the value of the degree so deduced to the several equations of condition formed from the subdivisions of the total arc, the probable error assigned by the calculation to each of those determinations is shown as follows:

Marennes and Saint Preuil . .	correction	+ 0″,351
Saint Preuil and Sauvagnac . .	„	− 0 ,336
Sauvagnac and Isson . . .	„	− 0 ,390
Isson and Geneva	„	+ 0 ,608
Geneva and Milan	„	+ 0 ,047
Milan and Padua	„	− 0 ,407

These corrections, assigned on the system of the least probable errors, must be regarded in each case as applicable to the sum of the errors arising from three sources; namely, in the comparison of the clocks with sidereal time at the *two* extremities of the arc, and in their comparison with each other by means of the intermediate signals: the average correction to each arc is 0,35″.

For the deduction of the Ellipticity from the combined values of a degree of the parallel and a degree of the meridian, MM. Brousseaud and Nicollet have employed the formula given by M. Puissant, in the memoir to which we have already referred.

$$\alpha = \frac{\frac{1}{2}(B - A\cos H)}{B\left(\frac{1}{4} + \frac{135}{\pi}\cdot\frac{\sin(\lambda-\lambda')\cos(\lambda+\lambda')}{\lambda-\lambda'}\right) + \frac{1}{2}A\cos H\sin^2 H}$$

wherein A is the mean degree of the meridian between the latitudes λ and λ''; B the mean degree of the parallel in the latitude H; π the circumference of a circle of which the radius is unity; and α the ellipticity. Combining then 77865,75 mèt. as the mean value of the degree of the parallel in the latitude of 45° 43′ 12″, with 111140,51 mèt., the mean degree of the meridian between the latitudes of Dunkirk 51° 02′ 09″,2, and Mont-Jouy 41° 21′ 46″,58, (which is the measured arc of the meridian passing through France,) the ellipticity is obtained $\frac{1}{273,3}$. Substituting, in the combination, the arc of

the meridian between Greenwich 51° 28′ 40″, and Formentera 38° 39′ 56″,11, of which the mean length of a degree is 111115.08 mèt., the ellipticity deduced is $\frac{1}{272,7}$. Such are the results of the comparison of the arc of the parallel in the latitude of 45°, with arcs of the meridian which intersect the parallel, and are measured on the same part of the globe with itself. If we would now compare it with arcs of the meridian measured in latitudes widely different from its own,—with that of India, between the latitudes of 8° 9′ 38″,39 and 18° 3′ 23″,64, of which the mean degree is 110628,60 mèt., we shall obtain by the same formula $\frac{1}{290,5}$; and if with that of Peru, of which the limits are 0° 2′ 31″ S. and 3° 4′ 32″ N., and the mean degree 110582,10, we obtain $\frac{1}{294,7}$.

An attempt to investigate *with precision* the Ellipticity which might best conform to the combination of this new element with preceding determinations, would obviously be to anticipate that period when the value of the degree itself shall be ascertained with the additional accuracy, which the prolongation of the arc to Orsowa, nearly doubling its present extent, will produce: in the meantime M. Nicollet has assumed the arithmetical mean between the four results which we have given above, $\frac{1}{282,8}$, as, provisionally, the indication of the general Ellipticity of the globe, afforded by the combination. Thus much we may at least infer, that as no one of the results approaches so small an ellipticity as $\frac{1}{306}$, and as it would require an alteration in the length of the degree of the parallel of nearly 30 mètres to produce $\frac{1}{306}$ as the mean result of the combinations, it is the decided tendency of this method of inquiry, no less than that of the Pendulum experiments, to be irreconcileable to that proportion between the earth's axes, with which it was so lately supposed that all methods of experimental inquiry were in accordance. We have now the result of the pendulum experiments $\frac{1}{288}$; of those meridian arcs which have been most accurately measured $\frac{1}{278,6}$, as

lately shown by Mr. Airey; and of the arc in the mean parallel, as combined with the principal of these, $\frac{1}{282,8}$.

We are struck by the progress that this great question, so long the subject of scientific research, is now making, when we remember that at the period at which the Pendulum experiments were published, their results stood alone as an indication, derived from an extensive combination, of a much greater Compression than was then almost established in the general belief; two years have not since elapsed, and already closer investigation, and experiments not then reported, have brought the evidence of two other methods of inquiry in approximation to its own; indeed, in the present state of the inquiry, the Pendulum result, which was the greatest, now appears the least. The differences, however, are only of that nature and amount, that we may reasonably expect will gradually give way to the further progress of experiment. The measurement of parallel arcs is carrying on by the states of the continent with great spirit. The Pendulum is now in the hands of almost every nation. The measurement of meridian arcs alone is stationary; and there can be no question that those which have been hitherto measured are insufficient for the determination of the figure of the earth. We rejoice that in this country the subject has engaged the attention of Mr. Airey; and we cannot but anticipate an increased interest in it from his discussion, published in the Philosophical Transactions of the past year.

The project of measuring an arc perpendicular to the meridian on the parallel which is common, or nearly so, to the capitals of France and Austria, appears to have originated in France; having been made the subject of a communication from the French to the Austrian government, in the year 1809, by which its conjoint execution was proposed, and was readily acceded to on the part of Austria.

The arc which has resulted from this proposition has its western extremity determined by the meridian of the observatory of Brest. From thence it crosses France to Strasbourg, intersecting the meridian of Paris a few miles south of

that capital. At Strasbourg, it enters the territory of Baden, which it crosses to that of Wirtemburg; and successively the territories of Wirtemburg and Bavaria, to those of Austria; connecting itself in its progress with the observatory of Munich. Proceeding onwards through Austria, it is successively connected with the observatories of Vienna and Buda, and is carried across all the countries subject to the Austrian dominion, until it reaches the frontiers of Russia, between Moldavia and Gallicia, where its eastern limit is for the present determined by the meridian of Czernowitz. The extent of the arc comprised between Brest and Czernowitz is not less than 30 degrees, being a twelfth part of the entire parallel. Its importance, therefore, is at the least not inferior to that of the arc of the mean parallel of which the amplitude does not attain 24 degrees. Nor are the operations less advanced; but as the accounts of them that have hitherto appeared are far less systematic, or complete in the details, than those of the mean parallel, we have been under the necessity of giving it the second place in the present notice. For the particulars with which we are acquainted, we are principally indebted to two Memoirs, printed in the eighth volume of the Mémorial Topographique et Militaire, by Colonel Bonne, of the Ingénieurs-géographes, the survivor of the superior officers of that corps intrusted with the conduct of the operations in France and Bavaria; and to the correspondence of M. Littrow of Vienna, with the Baron de Zach, published in the third cahier of the third number of the Correspondance Astronomique, &c., for 1822.

When, in 1809, the Austrian government acceded to the proposition of France, the geodesical operations which the measurement of the portion of the arc passing through Austria would require were already far advanced; having been undertaken in reference to the topographical map of the Austrian dominions. In France they were immediately commenced, and their execution between Brest and Strasbourg was completed in 1823, forming a suite of upwards of 60 triangles, resting on bases, measured near Paris for the centre, and near Brest and Manheim for the extremities of the arc passing through France. At the same period also the connexion

between the French and Austrian portions had been established under the direction of the intermediate governments.

In 1822, the measurement of the corresponding celestial amplitude was commenced by the Austrians, on the portion of the arc comprised between Munich and Buda, in Hungary. The method of determining the celestial amplitude on this parallel was essentially the same as on the arc of the mean parallel; that is to say, it was accomplished by comparisons of the sidereal time of selected stations, effected by instantaneous signals given at intermediate points. The mode of accomplishing the comparison, however, and the arrangement of the intermediate signals, differed greatly on all points of this parallel from that pursued on the parallel of 45 degrees: nor were the proceedings upon the French and Austrian portions of the parallel less different from each other in their details of execution, than both were from the proceedings on the other parallel which have been already described. Viewing the subject as interesting alike from its novelty, and from the probability that the method will be frequently and extensively resorted to hereafter, for the determination of differences of longitude, we shall briefly describe the variations which have taken place, and the practical inferences that present experience appears to warrant. Without doubt, theoretically, the most advantageous application of the method of instantaneous signals to the determination of the celestial amplitude corresponding to an arc of considerable extent, would be by the same arrangement of stations as was adopted on the parallel of 45°; namely, the total arc divided into portions by astronomical stations, each provided with a clock, accurately compared with the heavens by astronomical observations, and with each other by signals from one intermediate station, the apparition being registered directly by the clocks; but in the execution of the operations, it would be preferable, that, instead of the several portions of the arc being determined successively and independently, the whole should be accomplished at the same time, the signals being given and observed along the whole extent of the line on the same nights. By this means no other errors committed in the determination of time, except those at the extremities of the total arc, would influence the ultimate result; whilst, by

the time being ascertained with all due care at each of the stations, whatever interruption might take place in the continuity of transmission, from circumstances of weather or other casualties, would be confined in its effect to the spot at which the chain should be broken; leaving, in such case, the partial determinations, as well as those in which no such interruption should have taken place, available in the general conclusion. Such, in theory, may be considered the most advantageous arrangement. The practical objection to it is:—the number of persons, of competent astronomical qualifications, whom it requires to station simultaneously on different points of the arc. It was the very limited number of such persons that occasioned the *successive* determinations of the portions of the parallel of 45°, and which rendered the succession a consequence of necessity, not of choice.

Next to the above arrangement, is a second, scarcely inferior to it in the certainty, or in the accuracy, of the result, in which the whole line may be covered by the same number of individuals, but having amongst them a smaller proportion of highly qualified observers. It is the mode adopted by the Austrians on their portion of the parallel between Munich and Czernowitz: it consists in deciding, that every alternate observing-station shall be a station of transmission only; and furnishing it accordingly with chronometers, instead of an astronomical clock, and with the more simple apparatus by which the daily rates of chronometers are ascertained, instead of the transit, or repeating circle. The arc is still divided into portions, but these are of greater extent than in the former arrangement, inasmuch as every two stations at which the time is determined, comprehend between them *two* stations at which signals are given, and a third, intermediate between the two latter, provided with chronometers, and charged to make known the exact interval of time elapsing between the signals on either hand; for which purpose the rates of the chronometers may easily be sufficiently ascertained. If the operations be established along the whole extent of the line at the same period, the astronomical stations may still be employed, as in the former case, as stations of transmission only, and the total arc be chargeable with the errors only of the

extreme stations. This advantage, however, was not derived by the Austrians, who, apparently from the different states of preparation of different parts of the arc between Munich and Czernowitz, determined it in separate portions independently of each other. Thus, in May, 1822, the difference of longitude between the observations of Vienna and Buda was ascertained by explosions of powder, at the chapel of St. Rosalia, on a mountain on the frontiers of Austria and Hungary, seven and a half German miles from Vienna, the apparition of which was noted at Vienna, and by chronometers stationed on a mountain near Dotis, in Hungary; and by other explosions on the Nasnal, six German miles from Buda, the apparition of which was noted by the chronometers at Dotis, and at the observatory at Buda. The observations were made on the nights of the 12th, 21st, 22nd, and 23rd of May, giving, as the result of ten comparisons, 10′ 40″,699. A second trial was made in August of the same year, in which an alteration was made in the locality, but not in the arrangement of the intermediate stations, by which twenty-eight comparisons were effected of thirty attempted, on the nights of the 17th, 18th, and 19th; the result was 10′ 40″,7, which may be considered as identical with the former determination, and is a good specimen of the confidence to which this method of determining differences of longitude is entitled. In the same month, but not on the same days, the difference of the meridians of the observatories of Vienna, and Bogenhausen near Munich, was ascertained, by a similar arrangement of intermediate stations, to be 19′ 05″,202. No report has yet been made of the signals between Buda and Czernowitz.

The arrangement adopted on the French portion of the parallel is an extension of that introduced by the Austrians (of substituting chronometrical for astronomical stations) to all the stations intermediate between the extreme ones of the total arc, where alone consequently the absolute time is determined. It may be regarded as a mode of covering an arc of considerable extent, where there is no deficiency of individuals capable of noting correctly the apparition of signals by a chronometer, but with the smallest possible number of observers competent to determine absolute time,—rather than as, on the

whole, an eligible mode of procedure. A single link failing in the chain of transmission, nothing is accomplished, and the whole line has to be re-established for another attempt. Experience has fully shown, we think, the inconvenience and inexpediency of attempting too extended a transmission, without providing for the security of a part, should the whole transmission fail. In 1824, the determination was attempted between Strasbourg and Paris, having two intermediate stations of transmission, and, by consequence, three intermediate signal-stations: the signals were continued for ten successive nights, ten in each night, at each post, making altogether one hundred signals. And in the same year the arc between Paris and Brest was attempted by an arrangement altogether similar. The latter failed wholly, nothing whatsoever being accomplished; and in the arc between Paris and Strasbourg, six transmissions only succeeded, giving a result which it has since been found necessary to reject. The following year, 1825, both arcs were again attempted, two additional stations, one a signal, and the other a chronometrical station, having been introduced on the line between Paris and Brest; and a fresh arrangement of the stations, but not increasing their number, between Paris and Strasbourg. The number of successive nights was also increased to twelve, making one hundred and twenty signals throughout the line. Of these, thirty-five succeeded, on the line between Strasbourg and Paris; but between Paris and Brest the transmission again failed altogether on eleven nights out of the twelve, and the result obtained on the single night was not considered sufficiently satisfactory to prevent the necessity of an entire repetition of the whole in the summer of 1826. The details of the third and successful attempt have not yet been published; but we have learnt, that the precaution was adopted on this occasion of virtually dividing the arc into two portions, by an astronomical station midway between Paris and Brest.

The measurement of the arc, between Paris and Brest, gave rise to a similar undertaking between Paris and Greenwich, to determine the long-agitated question of the difference of longitude between the respective national observatories. This was accomplished in 1825, by Colonel Bonne and M. Largeteau, on the part

of the French government, and by Mr. Herschel and Captain Sabine, on that of the British. The arrangement of the intermediate stations was the same as that which we are now considering; viz. three signal and two chronometrical stations. The execution took place at the same period as the unsuccessful attempt of that year, between Paris and Brest, one of the signal stations (that nearest to Paris) being common to both arcs. In this case also, four nights only of the twelve succeeded, although, from the accordance of the partial results obtained on those nights, the general conclusion is considered as satisfactory, and as not requiring repetition.

It may not be without its use to collect, in one view, what experience has shown as to the proportion of powder proper to be employed in the signals; because, whilst on the one hand the quantity must be sufficient to insure the visibility of the signals, it is desirable also on the other to be guarded against excess, since the smaller the quantity of powder the more instantaneous is the inflammation. In the instructions given by the état-major-general, at Vienna (Ephem. di Milano, 1826, Appendix, p. 46,) to the Officers employed to make the signals on Monte Baldo, which were to be seen at Milan, a distance of seventy-two geographic miles, the quantity of powder directed to be used was $\frac{3}{4}$ of a Viennese pound, to be increased to a pound in unfavourable states of the atmosphere. The signals were seen at Milan, by M. Carlini, who makes no particular comment concerning them; but in the report of Professor Santini (*idem*, Appendix, p. 67,) by whom they were also observed at Padua, forty-three geographic miles from Monte Baldo, after noticing that the signals were so distinctly visible as to be observed by the naked eye, as well as with glasses, he remarks, that they could not be said to be absolutely instantaneous; that the progress of the inflammation was perceptible, and that the illumination lasted, from its first appearance to its total extinction, about a quarter of a second, or, perhaps, even one-third of a second; which obliged him to endeavour, as far as was possible, to appreciate and register the instant of the greatest splendour. We conclude, therefore, that, for distances of from forty to fifty miles at least, the quantity directed by the authorities at Vienna was excessive; and this is further confirmed by the fact, that

MM. Brousseaud and Nicollet employed, for distances from sixty to eighty geographic miles, half a French pound, and even between a quarter and a half, finding that quantity ample for visibility. By experiments made at St. Preuil, by M. Nicollet, we learn that, at distances of twenty geographic miles, the inflammation of so small a quantity of powder as the sixteenth of an ounce was distinctly seen by the naked eye; and that even the flash of the priming of a musket was visible, at the same distance, by the aid of glasses. It is related by the Baron de Zach, (Cor. Ast. 1822, p. 273,) that, in experiments made in 1804, the illumination made by six ounces of powder fired on Mount Brocken, was seen without glasses on the Keulenberg, a distance of one hundred and thirty geographic miles, although the Mount Brocken itself is not visible from Keulenberg, being below its horizon; a fact which, related upon less authority, might appear almost incredible. More general experience, however, has shown that the illumination alone, diffused by the inflammation, cannot be relied upon as a signal, but that it is necessary that the inflammation itself should be actually within the view.

The knowledge of the least quantity of powder that will secure visibility becomes the more important, when, from the absence of hills of sufficient height, it is necessary to give the signals an artificial elevation by means of rockets: this was the case on the whole extent of the arc between Brest and Strasbourg, except at a single station, Donon, where the arc is crossed by the chain of the Vosges; at all the other stations of this parallel in France rockets were employed, varying in diameter from 15 to 18 and 24 French lines, and calculated to carry eight ounces of powder to the respective heights of 200, 300, and 400 toises, previous to its explosion, which was rendered the more instantaneous by a contrivance in the rocket to carry the spark, by which it is fired, at once into the middle of the powder.

In a country, in which there are no hills of sufficient height to direct the choice of stations, and where, in order to avoid the too great multiplication of posts, the stations are necessarily taken not visible from each other, the selection may still, in some degree, be guided by the consideration, that those places are most eligible as stations of observation, which have a low hori-

zon in the two directions in which it is desired to establish the signals; in consequence of which a less elevation is required to be produced by the rockets.

To calculate this elevation, or the height to which a signal must be raised at a station A, in order to be visible at a post of observation B, if we take d = the difference of level between the stations; K = their distance apart; N = 0.08, the mean refraction; Z = the observed zenith distance of the horizon, viewed from B, in the direction A; and z = the zenith distance at which A would be visible from B, were it not for the intervention of the horizon; then is cot. $2 = \frac{dN}{K}$ 0.0000000658 K, all the measures being expressed in metres; and $z - Z = a$ is the vertical angle which the rocket must pass through to bring the signal in view from B; and from which angle the height in metres is found by K tan. a. The greatest height required on the French arc was about 600 metres, for which rockets of 24 lines diameter were employed.

If we would take an average of the distances on the parallel, intercepted between the meridian of the stations, so as to be in some measure guided in judging of the number of stations that an arc of given extent would require, we find 72200 metres for the average distance on the parallel of 45°; and 70370 for that on the parallel of Vienna and Paris, of which the German portion averages 73000, and the French portion (in which the countries may be considered as remarkably deficient in natural advantages for such operations) 67850 metres. These numbers are all between 36.5 and 39.5 geographical miles; and, of course, are something less than the average direct distance between the signals and the posts at which they were observed.

We learn from Colonel Bonne that the Heliostat has been introduced with success on the portion of the arc that passes through Baden and Wirtemberg, the disappearance of the reflection of the sun (rendered instantaneous by the rapid motion of an intercepting screen) being substituted as signals for the explosion of powder. We perceive also that the employment of M. Fresnel's lamp is recommended both by Colonel Bonne and Colonel Puissant, to answer the same purpose by night as the Heliostat does in the day; a recommendation enti-

tled to the more consideration, since the invention of Mr. Drummond's lamp, employed since 1825 in the geodesical operations in Great Britain, the light from which, compared with the light from M. Fresnel's, is in the proportion of 20 to 1.

We come now to the consideration of the ellipticity with which the measurements on this parallel correspond, so far, at least, as the authorities of which we have spoken enable us to draw a conclusion. Commencing with the arc between Buda and Munich, as the most easterly of which we have any particulars whatsoever, we are informed by M. Littrow (Corr. Ast. Zach. l. c.) that the geodesical distance measured between the observatories of Buda and Vienna, calculated on an assumed ellipticity of $\frac{1}{3\,1\,0}$, was found equal to a difference of meridians of 10′ 41″,292; and that the distance measured between Vienna and Bogenhausen (near Munich), computed on the same assumed ellipticity, was found equal to 19′ 05″,26; making together a geodesical amplitude between Buda and Bogenhausen, of 29′ 46″,552, on the supposition of the ellipticity being $\frac{1}{3\,1\,0}$. It is not difficult to learn from hence the value of the arc in metres from which this deduction must have been derived: by combining this number of metres with the true difference of meridians, or the celestial amplitude obtained by the fire-signals, 29′ 45″,9, we obtain the value of a mean degree of the parallel; by combining this again with the mean degree of the meridian between Greenwich and Formentera, we find that the operations on the parallel between Buda and Munich, taken in concert with those between Greenwich and Formentera, correspond to an ellipticity of $\frac{1}{2\,8\,2}$.

We find in Colonel Bonne's report the celestial arc between Bogenhausen and Strasbourg, 15′ 29″,42, apparently the most satisfactorily determined of any of the portions on either of the parallels. Unfortunately, we are nowhere favoured with the results of the corresponding geodesical measurement; nor with any deduction or inference, from which we might obtain it, by treading back the steps of a calculation. This is the more to be regretted, as it is the only result which is wanting to enable the combination of the three portions, viz., between Paris and Strasbourg, Strasbourg and Bogenhausen, and Bogenhausen and Buda, which would make a connected arc of between six-

teen and seventeen degrees. From this portion, therefore, we can draw no conclusion whatsoever.

We are informed by Colonel Bonne, that the difference of the meridians between Strasbourg and Paris, computed from the triangles on the assumed ellipticity of $\frac{1}{309}$, amounts to 21′ 35″,88; in which estimate we are enabled, in the manner before noticed, to acquaint ourselves with the value of the arc in metres from which it was originally made. The first attempt to ascertain the celestial amplitude of this arc was made in 1824, under the direction of Colonel Henry, when, of one hundred signals, six only were transmitted throughout the line, viz., three on the 25th of August, and three on the 26th. The six transmissions, however, presented so satisfactory an accord with each other, that, in the first of the two memoirs published by Colonel Bonne, in the Mémorial Topographique et Militaire, namely, in the one which records the proceedings up to the end of 1824, the presumption is expressed, that the mean result so obtained would scarcely be altered by a new attempt. The attempt was, however, renewed in the following year, under the direction of Colonel Bonne, Colonel Henry being dead intermediately, when the result of that year was proved to differ nearly eight-tenths of a second from that of the preceding year; of which it is stated in explanation, in the second memoir of Colonel Bonne, that the unfavourable weather of the preceding year had not permitted Colonel Henry to ascertain the correction of his clock with sufficient exactness. We have been induced to notice this circumstance, because it strengthens our view (to which we shall presently advert) of the expediency of providing some check, at least, upon the total arc. It would have been hard to have judged that an amplitude derived from the observations of two different days, the partial results of which, as well as the mean of each day, presented so satisfactory an accord, and in the report of which no circumstance indicative of doubt was mentioned, was inferior to other determinations, especially on the parallel of 45°, which have not been so tested by repetition: nevertheless, the error that repetition in this case discovered, was of a magnitude that, distributed on the whole extent of the arc, from Brest to Czernowitz, would still have very materially influenced the ultimate conclusion, not less indeed than eight in

the denominator of the fraction expressing the ellipticity. Nor could the discordance, which the comparison of so erroneous a result with other portions of the parallel would have manifested, have insured a repetition; since we observe the anomaly little less in amount which exists in the portion of the parallel of forty-five, between Isson and Geneva, has not been considered to require a re-examination.

The extension of the number of intermediate chronometrical stations, as practised in France, has given rise to a question of some nicety, of which the solution was not at first very obvious, but which has been well and clearly discussed by Mr. Herschel, in the account which he has given (Phil. Trans., 1826) of the operations in which he was engaged, for determining the difference of meridians between Greenwich and Paris. The desideratum which he has there supplied is the most advantageous way of employing a *broken* series of observations; that is to say, a series made on a line, on which there are two or more stations of transmission, and on which failures in the complete transmission have taken place occasionally at different parts of the line. To reject all but complete transmissions is obviously to reject a part of the evidence, when it is most important to preserve all that the observations will afford; and it does, in fact, frequently happen, that the cases in which a link fails, and which will give no result at all if taken singly, form the principal part of the actual observations, and being treated properly, are not less capable of yielding a correct result than the complete transmissions. Did the chronometers keep strict sidereal time, it is evident that the difference between any two of them would be constant; and that it would be indifferent whether the time at which they were compared with each other was earlier or later than the comparisons of each with the others next in succession in the line: that all the comparisons, at all parts of the line, on the same night, whensoever occurring, might, in fact, be regarded as simultaneous. The chronometers, however, do not keep strict time; which renders it necessary, particularly if the series be much broken, to take into the account their rates, and the intervals elapsing between the comparisons at different parts of the line. We may perhaps render this clearer by an illustration:—Suppose

three chronometers, A, B, and C; A and C keeping true time, B intermediate between the others, gaining five seconds in the 24 hours: suppose, further, that the three first signals given on any particular night are the only ones seen by A and B, and that in like manner these signals only are seen by B and C, but that those signals are the three last given on that night; the number of signals in each night being ten, the interval between each ten minutes, and the signals between B and C given always five minutes later than those between A and B. In this case, the comparison between B and C takes place one hour, fifteen minutes after that between A and B, B having gained intermediately on each of its neighbours $\frac{5+1,25}{24} = 0',3$. Now the sidereal time arriving from one extremity of the line has to be conveyed to the other extremity, by being successively transferred from A to B and from B to C, by means of their respective differences. But the difference between B and C, taken 1 hour 15 minutes after the comparison of B with A, differs 0',3 from what it would have been had the comparisons been simultaneous. The sidereal time will, therefore, be repeated from C, charged with the whole of the error arising from the rate of B in the interval of comparison, unless a correction, proportioned to the rate and interval, be applied to the differences. The rates of the chronometers adopted in this illustration are such as actually did exist in the operations between Paris and Greenwich; and but for the corrections applied, would have introduced a considerable error.

We consider, therefore, that Mr. Herschel has rendered a valuable service to those engaged in operations of this nature, by giving a short and easy method, whereby the result of such a broken series may be computed, with a due consideration of the respective rates and intervals, and without rejecting any observations but such as, having been seen only at a single post, must of course be wholly useless.

From the 120 signals given in 1825 between Paris and Strasbourg may be derived 35 results, either of complete transmissions, or from signals, between which the intervals were so small as to admit of their being combined, to represent complete transmissions. The mean of the 35 results is 21′ 35″,4. View-

ing, however, with Mr. Herschel, the impropriety of rejecting all those observations which did not admit of being so combined, Colonel Bonne has investigated the general result which the whole body of evidence will afford: the method that he has pursued for this purpose is not explained, but we may suppose that it is analogous at least to that of Mr. Herschel, because he states that all the intervals between the signals shewn by the chronometers were reduced to sidereal time, and corrected for the several rates. The result thus obtained, and which we must presume to be much preferable to the former, is 21′ 35″,55.

The clocks at Strasbourg and Paris having been compared with astronomical time, by means of different stars, the question of their right ascension becomes involved, and the differences in that respect that exist in the catalogues of Maskelyne, Bessel, and Pond. The amplitude of 21′ 35″,55, which is given by the mean of the three catalogues, when separately computed by each, is as follows:

Maskelyne . .	21′ 35″,55	21′ 35″,55.
Bessel . . .	21 35 ,62	
Pond	21 35 ,49	

For these differences there is of course no other remedy than to take the mean; but it would be a preferable course, on future occasions, to avoid them, by concerting to employ the same stars.

The amplitude 21′ 35″,55, combined with the value of the arc in metres deduced from the triangles, gives the mean value of a degree of the parallel, which, combined with the mean degree of the meridian between Greenwich and Formentera, corresponds to an ellipticity of $\frac{1}{288}$.

The celestial amplitude between Paris and Brest, having only been determined in the course of the last summer, is not yet reported.

We have had great satisfaction in learning that a communication very recently made by the French government to Russia, proposing the further extension of the arc from Czernowitz, through the Russian dominions, where the parallel passes north of the Caspian, has been favourably received; and that an examination of the country, preparatory to its being undertaken, is arranging for the ensuing summer, to be

executed by Russian officers, accompanied by one or more of the French officers, who have been engaged in the operations which we have described.

It is obvious, however, that even for the deductions to be drawn from the arcs of the parallel, the extension and multiplication of the measurements on the meridian are already become more requisite than of those on the parallel.

We merely avail ourselves of the occasion to express a hope, that before the operations on either parallel are considered to be concluded, the endeavour may have been made to obtain the celestial amplitude of each, by astronomical observations, executed in concert at the extreme stations. For arcs of small extent, there cannot be a doubt that the determination by signals is far preferable to every other method; but there must be a term in the extension of the distance to be measured, when the celestial reference from the extremities equals in value, and in still greater extensions must be regarded as preferable to the method by signals. We are not prepared to say which method might deserve the preference on arcs of the magnitude of those of Marennes and Orsowa, Brest and Czernowitz; because we could assign by conjecture only the degree of accuracy attainable in several months of diligent and concerted observation, to determine the difference of meridians of the extreme stations, by all possible phenomena, particularly by lunar transits and occultations. But we cannot doubt that the attempt, which would be far less costly than the operation by the other method, would lead to highly valuable conclusions, not only in producing just notions of the relative value of the methods; but in furnishing a result, which, in comparison with that obtained by the instantaneous signals, could not fail to be most advantageous and important to the arcs themselves.

E. S.

On the Probable Decomposition of certain Gaseous Compounds of Carbon and Hydrogen, during sudden expansion.

Some very singular appearances have been observed by Mr. Gordon, of the Portable Gas Works, which have led him to believe that chemical changes are occasioned by the sudden

expansion of oil gas, which do not happen when the expansion is gradual; a striking result of the change being the separation of carbon from the gas. The effect referred to is exhibited when oil gas, compressed into vessels by a power equal to that of 30 atmospheres, is suddenly allowed to escape through a small aperture into the air. It was first observed accidentally, in consequence of the derangement of the valve of a large apparatus, into which the gas had been compressed to 27 atmospheres. The gas escaped with immense velocity, and when an examination took place of what had happened, it was found that all the metallic part of the valve upon which the gas had rushed was covered with a black, moist carbonaceous substance, and the contiguous brick wall with dry, black carbon, the moisture in this case having been absorbed by the brick. Since that time, Mr. Gordon has repeatedly shewn the effect, by allowing the gas to rush out with very great violence from a portable lamp against a piece of white paper, which becomes immediately covered with black carbonaceous deposit.

The general conclusion is, that as the gas thus rapidly expands, a partial decomposition takes place, and carbon is separated. If this explanation should ultimately prove, by further experiments, to be true, it will be highly important, as affording an instance of the exertion of mechanical and chemical powers in those circumstances where they most closely verge upon each other. At present, we have but little knowledge of such phenomena, though the announcement in France of the production of several new compound bodies, possessed of peculiar properties, solely by the exertion of physical powers, may lead us to hope for an accession of information on the subject; that which we thought we had, was in part rendered uncertain by the contrary conclusions arrived at by Mr. Perkins and Dr. Brewster, the one believing that in a case of crystallization the effect was produced entirely in consequence of pressure *; the other, that pressure had been the only cause why bodies, otherwise ready to crystallize, had retained the fluid state.

A natural suspicion, upon first hearing of and seeing the results obtained by Mr. Gordon, was, that the rapidity of the current of gas had carried away a minute portion of the metal

* Philosophical Transactions.

from the surface of the valve past which it rushed, or of the interior of the air-way against which it was thrown, and that that metal had caused the stain upon the paper; but upon examination, this proved not to be the case; for the black deposit upon a card, when subjected to acids, remained insoluble, and when burnt and tested chemically, gave no traces of copper.

Further examination of the substance showed that it was not a pure carbon, but one of those compounds, containing a very large proportion of carbon, combined with a small quantity of hydrogen analogous to tar, pitch, or asphaltum, for it dissolved readily in the fluid hydro-carbons obtained by the compression of oil-gas. As these black carbonaceous compounds are formed in the process of making oil-gas, a suspicion cannot but arise, that the effect observed *may* have been produced by the current of gas having swept off small portions of such substances previously deposited, or slowly formed in the interior of the vessels at former periods; and have left them upon the wall in the accidental result, or upon the paper placed in the current of the gas, when the effect has been purposely shown.

It may, however, be remarked that, in experiments made in the laboratory of the Royal Institution, upon the fluid product obtained by condensing oil-gas at high pressures, it was observed that, after rectifying the products and separating the more fixed from the more volatile, that although they were perfectly clear and transparent at first, yet by spontaneous evaporation through the corks which closed the vessels, and after a lapse of time, chemical changes were produced; for, ultimately, there remained nothing in several of the receivers but a brown substance, heavy, adhesive, like honey or treacle, and in certain cases even almost solid. From the circumstances of the experiments, no hesitation could arise in concluding that a spontaneous chemical change had taken place; and it does not seem at all unlikely that a similar change, or one to a much greater extent, may have occurred *suddenly* during the rapid alteration in the mechanical condition of the gas in Mr. Gordon's experiment; the most condensible of the substances in the mixture of elastic matters which constitute oil-gas, being perhaps those which are most altered, and in that case Mr. Gordon's account of the phenomena would be correct.

Proceedings of the Royal Society.

In consequence of the lamented death of H. R. H. the Duke of York, the business of the Royal Society was not resumed until the 25th of January, when a paper was communicated by Davies Gilbert, Esq., "On the Expediency of assigning Specific Names to all such Functions of Simple Elements as represent definite Physical Properties; illustrated by an Investigation of the Machine moved by recoil, and also by some Observations on the Steam-Engine."

In this paper the author presses the necessity of distinguishing by separate appellations all functions which measure the intensity of physical properties, and adduces, in favour of his argument, the controversy which ensued soon after the application of mathematical expression to the laws of motion, when it was contended, on the one hand, that the weight of a moving body multiplied into its velocity, was the measure of the motion; and on the other, that the weight should be multiplied into the square of the velocity, to obtain such measure. At length it was ascertained that these views were not really discordant, and the terms *momentum* and *impetus*, respectively applied to them, ended the discussion. The author then observes, that neither of these terms is a correct measure of the effective action of machines, the criterion of which is the force exerted, multiplied by the space through which it acts. Mr. Watt expresses this numerically, in reference to the steam-engine, by the term *duty*; and the raising of a pound weight to the height of one foot has been made by him the dynamic unit. For this measure, Mr. Gilbert proposes the term *efficiency*, and retains the term *duty* for a similar function, indicative of the work performed; and by comparing these two functions, a measure of the actual work performed by any machine may be obtained.

Mr. Gilbert then illustrates the utility of this term in investigating the mechanical power of the recoil-engine, and shows the impossibility of economically applying steam on such a principle.

Some observations on the efficiency of steam, and on the comparative powers of different engines, concluded this communication.

At this meeting, Professor Jameson's name was ordered to be inserted in the printed lists of the Society, he being unable at present to attend in London for personal admission.

Thursday, February 1.

A paper was communicated by D. Moore, Esq., F. R. S., giving an Account of a new genus of Serpentiform Sea Animals, by J. Harwood, Esq., M. D., F. L. S., Prof. Nat. Hist. R. I.

This singular animal was taken up at sea by Capt. Sawyer, in lat. 62° N. and long. 57° W. It was in an exhausted state, apparently in consequence of having gorged a very large fish.

Dr. Harwood observes, that in several characters this animal resembles the ophidian reptiles, especially in the structure of the jaws, which are truly serpentiform, with the exception of the interarticular bones. From this resemblance, and from the large sac with which it is provided, Dr. Harwood gives it the generic name of Ophiognathus, with the following characters:—

Corpus, nudum lubricum, colubriforme, compressum, sacco amplo abdominali.

Caput, antice depressum; maxillâ superiore (paulo) longiore.

Dentes, in maxillâ inferiore, et ossibus intermaxillaribus; subulati retroflexi.

Maxillæ elongatæ, patulæ, dilatabiles (serpentium instar).

Lingua, vix conspicua.

Spiracula, ante et sub pinnas pectorales; magna.

Pinnæ pectorales, dorsales, analesque, radiis mollibus. Ventrales, nullæ.

Oculi minimi; propé extremitatem maxillæ superioris positi.

Cauda elongata, in filamentum apterum producta.

The author then enters into a detailed description of the animal, to which he gives the *specific* name *Ampullaceus*, and which is illustrated by three drawings.

G. Poulett Scrope, Esq., was admitted a Fellow of the Society.

Friday, Feb. 8.

Sir Everard Home communicated a paper, entitled "An Examination into the Structure of the Cells of the Human Lungs, with a view to ascertain the office they perform in Respiration; illustrated by microscopical observations." By J. Bauer, Esq., F.R.S.

The author's chief object in this paper appeared to be, the disproval of the usually-accepted chemical theories of respiration, and to show that air, or at least oxygen, is actually absorbed in that function. This view of the subject he principally founds upon the anatomical structure of the air-cells, respecting which some new facts are brought forward, to which we shall take another opportunity of calling the attention of our readers.

Proceedings of the Royal Institution of Great Britain.

The Evening Meetings of the members of this Institution were resumed on

Friday, January 26th,

when Mr. Faraday gave "A General Account, accompanied with experimental illustrations, of the late Extension of our Knowledge relative to Magnetism, founded on the discovery of M. Arago, of the effects of metal when in motion."

The magnetic phenomena with which we were acquainted prior to the year 1818, consisted of the various effects produced by the action of natural and artificial magnets upon each other, and upon the metals iron and nickel, with some effects, very feeble and uncertain, (as to their origin), produced by the mutual action of magnets and bodies in general. In the year 1819, Oersted discovered that a current of electricity produced magnetic effects of an extraordinary kind. Upon the extension of the discovery by himself and others, it was found that the current was able to influence the magnetic needle, and permanently to magnetize steel bars, but that the power was disposed in a manner entirely unknown till then, and best expressed by the term, first applied to it by Dr. Wollaston, of Vertiginous Magnetism. In the year 1821, Seebeck first discovered such an arrangement of matter, that a metallic ring could be made vertiginously magnetic in every part, in a manner so analogous to that occasioned by a current of electricity, that though no other evidence of the electricity could be obtained than the existence of the magnetism, still it was generally admitted to be sufficient, and the dependance of the latter upon the former inferred. It was in 1824, that M. Arago added an entirely new fact to this branch of knowledge, by showing, that when a magnet was brought near a plate or ring of copper, or other substance not usually considered magnetic, and the one made to assume a rotary motion, that it immediately exerted a powerful influence on the other, which it had not whilst in a state of rest, and either tended to make it move, or was itself retarded. The effect was shown by making magnets and plates revolve or vibrate near each other. A horse-shoe magnet, weighing several pounds, was made to rotate rapidly by a thin copper plate; and a circular copper plate, which, being suspended according to Mr. Sturgeon's method, by an axis not in the centre of gravity, would vibrate, when moved, 60 or 70 times before it acquired a state of rest, would not vibrate more than six times when

inserted between the poles of the same magnet. The experiments by Messrs. Babbage, Herschel, Nobili, Christie, and others, were referred to, and M. Arago's objections to the theory which has been advanced stated and illustrated. The opinion entertained has been, that the magnet produces poles of an opposite nature to its own at the parts where they approach nearest to the copper, and that these poles are not assumed and destroyed instantaneously, but require a certain portion of time for their formation and destruction, in consequence of which they exert an attractive force on the poles which are passing over and from them, and drag, as it were, after them. M. Arago, however, shows, by the use of the dipping needle, and by long magnetic needles suspended to balances, that the force brought into action is not an *attractive*, but a *repulsive*, one; and states his original objection to the same theory, when it first suggested itself to him, by inquiring how it is that a metal, which, like copper, cannot, when in a state of rest relative to the needle, deflect the latter a single second, can, when in a state of motion, carry it 90° or more.

An Ornamental Lamp, recently constructed by Mr. Bartholomew, was placed upon the library table at this meeting. Some very elegant transparent paintings and spiral devices were kept in rotary motion, by the action of the current of heated air issuing from the chimney of the lamp. The contrivance appears well adapted to a number of purposes of ornamental illumination. A Night Lamp was also shown, invented by Mr. Blackadder. The flame is supplied with oil by capillary attraction, independent of the use of a wick.

Numerous Books, presented to the Institution by the Duke of Buckingham, Mr. Smith, and others, were also upon the table.

Some specimens of Dried Plants, from Massachusetts, were presented by Mr. Sharpe, of Boston. They were in the form of longitudinal cakes, about eight inches long, four wide, and two in thickness: each cake consisted of the leaves or flowers of a particular herb, which, after being dried and prepared in a peculiar manner, had been then accumulated and pressed together in these forms. The colours were not much injured, and the odours were strong and pure: prepared in this way, it is said that the herbs can be preserved unchanged for a great length of time, whilst they are rendered very portable. The specimens consisted of Thorough-wort leaves, *Eupatorium perfoliatum;* Spear-mint, *Mentha viridis;* and Elder flowers, *Sambucus nigrus.*

The secret of preparing these vegetable cakes is said to be possessed exclusively by the peculiar sect of people called the Shaking Quakers, and from whom the specimens in question had been obtained.

Friday, February 2nd.

" An Account of the Extensive Applications lately made in Paris of the Chlorides or Chlorurets of Lime and Soda, as disinfecting agents," was given by Mr. Alcock. It appears that the well-known power possessed by chlorine, of destroying putrid or infectious miasmata, is retained by it, even when it is in combination with lime, in the substance usually known in this country by the name of bleaching powder; or when in a similar state of combination with a weak alkaline solution. If the bleaching powder (chloride of lime) be sprinkled about a place in which putrescent or infectious vapours are floating, they soon disappear; or if it be dissolved in water, and then cloths be dipped in the solution, and afterwards brought into the infected atmosphere, they produce the same good result. These properties, combined with the innocuous nature of the chloride itself, renders it available in many cases where disinfection is required, where the use of free chlorine would be attended with inconvenience. Thus, in disinterring a corpse for the purpose of judicial inquiry, a cloth dipped into the solution of the chloride, and placed over the body for an hour or two, will prevent the developement of unpleasant effluvia; or in descending into pits, sewers, or cesspools, if the air breathed be passed through a sponge imbibed with the same fluid, the person can remain some time without injury in situations where others, not so provided, would infallibly be destroyed. The application of the compound or mixture of chlorine with solution of carbonate of soda* to ulcers or putrescent sores, was also lightly touched upon, and the good effects stated to have been obtained by others confirmed by Mr. Alcock.

The Library Tables were furnished with a very elaborate pen and ink drawing of a Holy Family, by Mr. Train; with numerous books presented by members and friends of the Institution; with specimens of new and unpublished works; with scarce manuscripts and books; and with a specimen of deadly vegetable poison from Assam, used by the Assamese to tip their arrows and spears. This specimen consisted of several small amylaceous roots in a dried state, which, when cut open, were found to be intersected with

* Which has received the name of chloruret of oxide of sodium.

darker-coloured portions. The roots were secured in a delicate little cane basket, and have not as yet been examined with respect to their powers or composition.

Friday, February 9th.

Mr. Ainger delivered a Discourse, the subject of which was, "The Principle of Security in the various kinds of Locks:" the following is an outline of this communication:—

The means of giving security to locks are of two sorts. The first consists in numerous obstacles (commonly called wards) to the passage of the key, which requires, therefore, a peculiar form to evade them.

The second consists in a number of impediments to the motion of the bolt; those impediments being so contrived that their absolute and relative positions must be changed before the bolt can be withdrawn.

Means of the first class are defective, because a surreptitious instrument need not thread the mazes of the obstacles or wards; it escapes them by taking a path on the outside of them, which is unavoidably left for the passage of the extremity of the key. Complexity in the form of the wards, therefore, does not increase security, neither does it add to the difficulty of imitating the key, because in all keys of this sort nearly two-thirds of the metal is superfluous, and requires not to be copied.

Means of the second sort are the most ancient, having been known in Egypt above four thousand years, as was inferred by M. Denon, from some sculptures on the great temple at Karnac, representing locks similar to those now used in that country, in which means of this sort are employed. The bolt, and a fixed part of the lock are each pierced with any number of holes, arranged in any chosen form, those in the bolt and in the fixed part coinciding when the bolt is locked. These holes are occupied by pins which are contained in the fixed part and descend into the bolt, so as to prevent its motion till they are removed wholly into the fixed part. This is effected by a key having the same number and arrangement of pins, and of such a length that they elevate the ends of the pins in the lock to the plane of motion between the bolt and the fixed part. This key is introduced laterally through a long tube, at the end of which it acts vertically upon the pins, whose position, therefore, it is difficult to ascertain.

This principle of security was not generally known in this country or in Europe, till it was re-invented by Mr. Barron, and applied by

him in conjunction with wards to the security of a lock, for which he obtained a patent in 1774. The same principle was afterwards adopted by Mr. Bramah, without the assistance of wards or obstacles to the key, but the mode of application was very different from the Egyptian. In the latter, the security arose from a concealment of the number and position of the impediments; in Mr. Bramah's, these were discoverable on inspection, and the security depended on the various degrees of motion which the several impediments required before the bolt could be moved.

These gradual improvements induced a corresponding attention to the art of picking, which would have been fatal to the reputation of the new locks, but for the almost simultaneous invention of a very simple and costless alteration, by which the required position of the impediments is rendered utterly undiscoverable.

There are a few contrivances which do not come within either of these classes: such as the detectors, which give notice of any attempt to introduce a false instrument, and the sliding curtain of Mr. Lawson, by which the key-hole is so perfectly closed, during the act of unlocking, that it would be impossible to move the bolt while a pick remained in the aperture.

Locks have also been made which required that the key should be a powerful magnet; others in which an unusual and complicated motion must be given to the key; others in which an improper key or instrument would fire a pistol or ring an alarum.

These expedients are, however, more curious than useful. There is no difficulty in making a lock perfectly inviolable, and nearly all those of any estimation now before the public are, when well made, quite secure.

Models and diagrams illustrating the two principles or means of security, as also of the methods of picking, together with specimens and models of the Egyptian and nearly all the modern locks, were exhibited in the Lecture Room.

It was stated, in the course of the evening, by Mr. Trevelyan, a gentleman of Cornwall, that a lock, resembling the Egyptian, is used in that county; that he has also seen it in the Faro Islands, to both which places it was probably taken by the Phœnicians.

A specimen of a fungus, separated by Mr. Trevelyan from a beech tree, was laid upon the table. The whole of its upper surface was covered by a layer of brown resinous matter, which extended also a little way round the edge over the under surface. When separated by a knife and examined, it was found to be combustible, with

much flame and smoke, and soluble in alcohol; insoluble in water; nearly tasteless, and, indeed, to have all the general properties of resins.

Several books, presented to the Institution, were laid upon the table, with numerous specimens of works still in the press, and some rare copies of ancient works.

Friday, February 16th.

Mr. Brande gave an account of the art of making dies for medals and coinage. (See page 116.)

A specimen of the Meleagris Ocellata, of Cuvier, Dindon Oeillé, or Honduras Turkey, was placed upon the table. As this very brilliant bird is not only very rare, but comparatively recently described in the Planches Coloriées of Temminck, a work in the hands of comparatively few, we think that we shall serve both the objects of this Institution, and those of the science of natural history, by inserting part of the description therein contained, which is from the pen of Baron Cuvier.

"*Meleagris Ocellata.* (Cuvier.) This magnificent bird unites the singular form of the turkey to a brilliance of colour which hardly yields to that of the peacock. Its height and carriage are the same as in the common turkey; but its tail is not so broad; the beak is also surmounted by a caruncle; the head and neck, two-thirds down, are naked, and coloured blue and red; upon each eyelid are five or six fleshy tubercles, and on the middle of the head five or six others; it has no vestige of the bunch of coarse hair on the breast, which characterises the male domestic turkey. All the feathers resemble in form those of the turkey; the feathers on the lower part of the throat, upper part of the back, the scapulars, and those on all the inferior part of the body, are of a bronzed green colour, and bordered with two lines, the inner black, the outer golden bronze; those on the middle and lower part of the back have the same distribution of colours, but are more beautiful, their green bronze gradually, in descending the back, becoming of a sapphirine blue, which, according to the reflections of the light, change into emerald green; and their bronzed borders, enlarging more and more, exhibit on the superior part of the back the brightness of gold; and towards its base, still increasing in extent of surface, they present a reddish copper colour, little inferior in brilliancy to that of some humming-birds, which effect is still height-

ened by their being separated from the rest of the green and blue, by a line of velvet black. The feathers of the tail coverts have those parts, which are least exposed, of a gray colour, but the gray is rendered more conspicuous in such as approach the extremity of the tail, in which, as in the true tail feathers, the extremities, having their blue and green parts surrounded by a black circle, and bordered on their outward edge with a beautiful golden green, represent the eyelike embellishments of the pavo bicalcaratus; there are four transverse rows of these eyes, separated by gray; the lesser wing coverts are of emerald green, with a narrow border of black; the greater coverts of the secondaries are of metallic copper, with golden reflections: internally, near the quill, they are emerald green; externally, grayish white: the primary coverts are blackish brown, with narrow oblique bands of white, as are also all the quills, except the external borders of the last primaries, and nearly all the secondaries, which are white: the secondaries nearest the back are tinted with golden green; the tail has fourteen feathers, and is round at the extremity; the feathers of the thighs are blackish; the legs are rather longer and stronger than in the common turkey, and have much larger and more pointed spurs; their colour is a fine red."

The most beautiful wild turkeys are entirely destitute of eyelike spots, and have the ground of their plumage of a bronze colour, changing into copper, each feather having a broad dark border, and another narrower border of a lighter colour; the tail is also composed of longer and stronger feathers.

The specimen placed on the table was also sent alive from the Bay of Honduras, as a present; it arrived in this country, in February, 1818, and, dying on its arrival, was stuffed, and placed in a room, in an elevated situation, where it remained unnoticed until the present time. It is also a male.

A third specimen is to be found in the Museum of Natural History, at Charleston, South Carolina; but the neck is so imperfect, as to have caused Mr. Say, the naturalist, to reject a memorandum which he had taken from it, with the view to publication, some years antecedently to the description, and thus to lose the credit of having been the first to make this brilliant bird known. The specimen is also a male, and from the great length of its spur, and the more decided development of the eyelike markings on the feathers of the lower part of the belly, as well as on the tail coverts, it is supposed to be a bird of mature plumage.

Since the specimen has remained at the Royal Institution it has

been visited by many persons, and among them one gentleman who had seen the identical bird alive at the Bay of Honduras, in the latter part of the year 1817.

He states, that these birds are found in the interior in the woods, that they are very rare and remarkably shy. That two were brought to the Bay, in the latter part of 1817, where he saw them in the possession of the gentleman by whom it has been ascertained that the specimen was sent to England, and that one of them was killed by running against the bars of the place of its confinement, when approached to be viewed. He cannot say positively whether the birds were taken by the woodcutters, or by the natives, but he rather believes by the woodcutters.

There is also reason to believe that another specimen was taken alive to Philadelphia in the year 1804, where it met with a similar fate to the last, being killed by running against the bars of the place of its confinement.

When it is added, that a gentleman, resident thirty years in the Bay of Honduras, never saw a single specimen of this bird; and that Charles Buonaparte, who has so distinguished himself as a naturalist, by his able continuation of Wilson's Ornithology, has recently sent persons thither expressly to procure a specimen, and that without success, it is hoped that this attempt to collect information in regard to it will not be without interest.

Lastly, when we consider how completely we have succeeded in domesticating the common turkey, as well as the great variety of other birds, with which our farm-yards abound, and which, in their wild state, were probably as shy as these, it may not be too much to hope, that this notice may fall into the hands of some of those who may hereafter have the means of procuring either the birds or their eggs, and by leading them to make attempts at their domestication, become the indirect means of making another most brilliant addition to such as have thus become more peculiarly subservient to the uses of man.

Friday, February 23rd.

Dr. Harwood gave, in the Lecture Room, "A Description of the Anatomy and Habits of several Species of the Seal:" the outline of this discourse, with which he has favoured us, will be found in another part of this Journal. (See page 71.)

General Tolley presented some interesting specimens of Egyptian antiquities to the Museum of the Institution.

Friday, March 2nd.

Some observations on the Principles of the Structure of Language, communicated by a Member of the Royal Institution, were read and illustrated by Diagrams by Mr. Singer, the Principal Librarian of the Royal Institution. An abstract of these observations, for which we are indebted to the author, will be found in the present Number. (See page 163.)

Friday, March 9th.

Some observations upon Ship-building, illustrated by Models and Drawings, were delivered in the Lecture Room, by Mr. Holdsworth. The remarks offered this evening were merely preliminary to a more extended discussion of the subject.

In the Library, a portable gas lamp was exhibited, furnished with the condensed gas obtained by the decomposition of resin, an invention for which a patent has lately been obtained by Mr. F. Daniell, and which promises to be of considerable importance in an economical view. The gas is extremely free from smell, and appears to surpass even oil gas in brilliancy.

Proceedings of the Horticultural Society.

January 2nd, 1827.

A PAPER was read upon the grafting pears upon quince stocks. The writer strongly called the attention of the Society to the importance of this practice, and pointed out the great advantages attending it. It is really surprising that English gardeners should so long have neglected a practice which has long been followed in France, and to which the excellence of French pears is, in a great degree to be attributed. The quince used as a stock, has the property of stunting the growth of pears, of forcing them to produce bearing branches, instead of sterile ones, and of accelerating the maturity of the fruit. No small garden should contain pear-trees grafted in any other way, nor any large garden be without them to a considerable extent.

Various seeds were distributed to the members present, as is the custom in this Society, and a number of different fruits were exhibited. Of these the gloux morecæa and the bruni rance, new

varieties of pears of the highest excellence, obtained by the perseverance of our Flemish neighbours, offered a gratifying example of the important results to which industry and patience, combined upon scientific principles, are sure to lead in horticulture, as well as other arts.

January 18th.

No meeting was held, in consequence of the decease of his Royal Highness the Duke of York, one of the few honorary members of the Society.

February 6th.

A number of short papers upon various horticultural matters were read. There was laid upon the table a specimen of coarse netting, for protecting fruit-trees, that had been manufactured in North Wales, at a very low price. This was the best material that has yet been produced for the purpose, and less than half the price of the cheapest of all similar manufactures.

Various flowers and fruits of the season were exhibited, and cuttings and seeds of choice fruits and vegetables were distributed among the members.

February 20th.

A paper was read upon the cultivation of camellias in the open air, in Yorkshire, and another upon metallic hot-houses. The latter, by Mr. William M'Murtrie, gardener to Lord Anson, and a very experienced cultivator, excited great attention. The author pointed out the great disadvantages attending hot-houses constructed of iron, if to be used as forcing-houses; showed them to be more expensive, and not more durable than houses of wood; and finally, declared his belief that they were, if considered as forcing-houses, totally inapplicable to such objects. In this he may be right; but with regard to iron houses for objects of ornament, and for the cultivation of flowering, and perhaps tropical fruiting-plants, we apprehend that there can be no doubt that they are, under good management, of the greatest advantage, while it must be admitted that, under careless or unskilful management, they are very much the reverse. In construction and maintenance they are expensive, and in management they are uncertain; but for appearance, and for promoting the health of the plants they contain, they are unrivalled.

An account was laid before the meeting, of a Meteorological Journal, which had been kept in the garden, for the year 1826. This was stated to have been kept upon the plan recommended by Mr. Daniell, in his *Meteorological Essays*, and appeared to have been managed with much care. Its contents will not bear an abstract, but we may mention that, from the results of the whole year's observations, it appeared that, in 1826, the mean dew point was 3.2, also the mean of Mr. Howard's twenty years' observations, and the mean temperature 2.5 above the same.

A large collection of the best late varieties of the apple, as grown in America, was exhibited. It was a remarkable circumstance that, while these fruits are unusually handsome, none of them, with the exception of the Newtown pippin, were, although sweet and pleasant, comparable to our fine European apples; and yet the Newtown pippin, the only good variety, is as much superior to any variety of apple known in Europe, as the others were inferior.

March 6th.

A paper from the Society's gardener was read, upon the best method of forcing rhubarb for tarts, and fine specimens of the leaves, forced in the manner described, were placed upon the table. The method was simply this: The seed is sown in a rich border, in the first week in April; the young plants are kept thin and clean during the summer, and before the growing season is fully over, they are taken up, put into common forcing-pots, three in each, and placed in a shaded border till they are wanted. In January or February they are put in the forcing-house, and submitted to a very gentle heat. This is the most simple, effectual, and certain mode of forcing rhubarb yet known.

Seeds of various curious vegetables were given away, and some new varieties of Chinese camellias, from the Society's garden, were placed on the table for exhibition.

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Micrometrical Measurement of Saturn, and of Jupiter and its Satellites.*—These measurements were made at Dorpat, by M. Struve, with the great achromatic telescope of Fraunhofer, and a micrometer furnished with the threads of a spider's web. By correcting the relative measures of Saturn and his double ring which were obtained, to the mean distance of that planet, M. Struve brought out the following values in seconds of a degree, and decimals of the second*.

External diameter of the outer ring	40″.215
Internal diameter	35.395
External diameter of the inner ring	34.579
Internal diameter	26.748
Equatorial diameter of Saturn	18.045
Width of the outer ring	2.410
Width of the inner ring	3.915
Interval between the two rings	0.408
Interval between the planet and inner ring	4.352

By combining the observations with certain results given by Bessel, M. Struve deduces that the inclination of the plane of the ring to the ecliptic is 28′. 5′.9, supposing the ring to be of insensible thickness, which agrees within 28′.2 with the result obtained by M. Bessel, with much less powerful instruments. But if Schröter be correct in his supposition, that the ring is 0″.125 in thickness, then the inclination will be only 27° 55′.3.

M. Struve observes, that he has not been able to trace any subdivisions of the ring. The external ring is sensibly less luminous than the inner ring. The inner ring also appears rather pale on the edge towards the planet, and not so distinctly limited as on the outer edge, circumstances which induce the supposition that the internal edge is less regular than the outer. No distinct spot could be observed on the ring, by which to ascertain its rotation. The five well-known satellites were well observed by the instrument; the sixth was seen several times; the seventh, discovered by Herschel, during the disappearance of the ring, never. The fourth presented a small disc, with a diameter of about three-fourths of a second.

Measurement of Jupiter.—The whole of the observations reduced to the mean distance of the planet, gave†,

Equatorial diameter of Jupiter	38″.442
Polar diameter	35″.645

* These angular measurements may be converted into measures of length, by observing, that, at the distance of Saturn, a second of a degree corresponds to about 1600 leagues of 25 to a degree.

† The second of a degree corresponds, at the distance of Jupiter, to about 870 leagues.

from which it would appear that the flattening of the planet is 0.078, or $\frac{1}{13.71}$, the larger diameter being taken as ninety. M. Struve has ascertained, by direct measurement, that the irregular appearance of ellipticity sometimes presented by the disc of this planet, is merely an optical illusion.

Of the satellites, the third had evidently the largest diameter, and was nearly the same in that respect as the fourth, but it is decidedly inferior in luminosity to all the others, and sometimes had a very pale appearance. The first satellite is rather larger than the second. The following are the results obtained by actual measurement :—

Diameter of the	first satellite	1.″018
„	„ second	0.914
„	„ third	1.492
„	„ fourth	1.277 —— *Bib. Univ.* xxxiii. 97

2. *Diamond Microscope.*—Of all the various substances furnished, either by nature or art, the diamond seems to be that most pre-eminently calculated to form small deep lenses for single microscopes, possessing a most enormous refractive power, combined with a low dispersive one, together with a very little longitudinal aberration. Mr. Andrew Pritchard, 18, Picket-street, Strand, has succeeded in forming a very thin double convex lens, of equal radii, and about 1-25th inch focus, from a very perfect stone of the finest water. Its polish is very beautiful, and, by its strong reflective power, at once informs us of the peculiar and invulnerable material of which it consists, while the large angle of aperture which it bears attests the faintness of its spherical and chromatic aberration. It appears from experiment, that, though the refractive power of different stones varies considerably, if a diamond and a piece of plate-glass are ground in tools of the same figure and radius, the magnifying power of the former will surpass that of the latter, at the rate of eight to three ; so that if the power of the glass-microscope should be 24, that of the diamond one will be 64 ! What a lift does this give us in the construction of deep single microscopes ! If a diamond was ground in the same tools which had produced a glass-lens of 1-75th of an inch focus, (which it would be very possible to do, and which we believe Mr. P. would undertake to effect,) it would turn out about 1-200th of an inch focus ; but this is by no means the only valuable property it would possess ; for Mr. G. Francis has been at the pains of calculating the value of the spherical aberration of a plano-convex diamond, with its curvature exposed to parallel rays, and finds it only 0.949 of its *thickness*, while that of glass is known to be 1.166, *cæteris paribus*. This difference would be very considerable, even if the thickness of a glass and diamond lens of the same focus and diameter was the same ; but this is, of course, far from being the case, owing to the immense refraction of the diamond, which gives a very short focus, with a

very shallow curve, and proportional thinness. If a diagram is made of the curves of any two lenses of the *same* penetrating and amplifying power, formed of glass and adamant separately, it will at once show what their spherical aberration amounts to; being enunciated in terms of their thickness, as stated above, and which of course is the same, whether they have their curved surfaces exposed to parallel rays, or their flat sides to divergent ones, from a radiant point in their focus, when used as magnifiers. If we select a hemisphere of glass, having its aperture equivalent to its focus, which is absolutely necessary for exhibiting certain test objects, and calculate the longitudinal aberration of a diamond of the same power and calibre, it will be found less than one-sixth of that of the glass! Again, the chromatic aberration of the adamant is very little more than that of a drop of water of the same radius, which is scarcely felt at all, when the stone is used only as a single microscope; so that we thus obtain a kind of *natural aplanatic lens*, capable of being carried to an enormous power, which is, in fact, the very *beau ideal* of the single microscope, necessarily the most perfect instrument of all, when divested of aberration. Of all the improvements in microscopes which have been originated by Dr. Goring, this is perhaps the most important, and the most likely to extend discoveries in minute nature.

As many individuals have chosen to doubt of the possibility of working diamonds into spherical curves, Mr. Pritchard will be proud to exhibit his instrument to amateurs of microscopic science.

3. *Results of Observations on Falling Stars, made by* Professor Brandes, of Breslau.—i. Falling stars move in all directions, relative to a vertical line; but the number which approach the earth is greater than that of those which move from it. From this it would appear to result that, during the short time for which they are visible, they are subject to the attractive power of the earth.

ii. Let the real direction according to which these bodies move be ascertained, being referred each time to that of the motion of the earth at the time of observation; then let a mean be taken of these partial results. If the results are sufficiently numerous, the direction which they will indicate will be diametrically opposed to that of the motion of the earth.

Falling stars undoubtedly have a real velocity; but it appears to result, from the preceding observations, that the greater part of their apparent velocity is a mere illusion, dependant upon the motion of the earth. May it not be worthy of remark, that we arrive at a new proof of the movement of our globe, by observing these fugitive and inconstant phenomena?

I have no occasion here to call to mind that the true line of motion of the meteor in azimuth and in height, requires the comparison of simultaneous observations made at distant places.—*Ann. de Chimie*, xxxiii. 413.

4. *Experiments on the Resistance of various Bodies to rupture, caused by Longitudinal Tension, by* M. Navier.—According to these experiments, iron has not its tenacity increased when laminated, as happens when it is drawn into wire. Six experiments on iron-plate, with the power applied in the direction of the length of the sheets, indicated a mean force of 41 kilogrammes for each square millemetre* of the transverse section. Four experiments on iron-plate, drawn perpendicularly to the length of the sheets, indicated a force of 36 kilogrammes.

Two experiments on sheet-copper indicated a force of 21 kilogrammes for each square millemetre.

Six experiments on laminated lead indicated a force of 1.33 kilogrammes for each square millemetre. It may be concluded, that the lead resists less for equal sectional surfaces, as it is rendered thinner.

Seven experiments on tubes and rods of glass indicated a force of 2½ kilogrammes per square millemetre.

Generally the iron began to lengthen sensibly, and to alter, with charges equal to about two-thirds, at least, of that required to effect the rupture. The same effect happened with copper, charged with a weight equal to half that required to occasion rupture. With lead, the effect was produced by rather less than half the final charge. These three substances presented very different appearances during and after rupture. The lengthening of the iron before it broke was irregular, and varied from 1-20th to 1-10th of the original length. The copper lengthened about 2-5ths of its first length before it broke, its other dimensions diminishing in consequence. The lead increased by about 1-10th of its original length, with a force a little less than sufficient to break it; but with a slightly-increased charge it gradually and progressively diminished in breadth and width, and when separated, the two parts presented a kind of cutting edge, by the gradual diminution of thickness, whereas the other substances broke suddenly with a transverse fracture.——*Annales de Chimie*, xxxiii. 225.

5. *Magnetism of Watch Works corrected.*—It is an important matter with watch and chronometer makers to neutralize concentrated magnetism, in the most minute parts of the steel works of time-keepers, as chronometers, &c. without the aid of heat. The endeavour to perform this is usually made by touching the part magnetised with the similar pole of a small magnet; but so unsuccessful is the operation generally, that it is avoided as often as possible. Mr. Abraham was induced, by the consideration that "time-pieces generally become actively magnetical, without being brought into contact with the agent that concentrates the fluid," to conceive that the magnetism developed might be dissipated in a similar way, and

* A millemetre is 0.03937 inches; a kilogramme is 15406 grains.

at last succeeded to a degree which had been previously unknown to chronometer makers. The process consists in dipping the part to be divested of magnetism, as a balance-wheel, into fine steel filings, and then presenting a fine magnet to the part clothed with filings, at the distance of from one inch to a quarter of an inch, according to the power to be neutralized. It will immediately be perceived whether the polarity of the magnet be of the same kind as that in the apparatus; if so, the filings will gradually fall from the part as the power becomes neutralized. When all the filings have fallen from the part submitted to experiment, dip it again into the filings, to prove whether it has acquired opposite polarity by remaining too long in the vicinity of the magnet; if that be the case, present the contrary end of the magnet at a distance in proportion to the power to be diffused.—*Trans. Soc. Arts*, xliv. 19.

6. *Peculiar Construction of Shot Cartridges.*—An ingenious construction of shot cartridges has been devised by Mr. Jenour, for the purpose of shooting at much greater distances than can be accomplished in the present way of discharging guns. It deserves to be examined, and its advantages, if it possess any, proved by repeated experiments, for which reason the following brief account of it is given:—The unwinding of a cord or twine, a chain or wire from a spiral groove infolding the charge of shot, constitutes the chief principle, by means of which they are thrown from the gun in a mass, and dispersed at various distances. Two or more pieces of metal are so constructed as, when bound together by the twine, &c. to form a cylindrical case, inclosing the charge of shot corresponding to the size of the gun, and having a regular spiral groove from end to end, similar to a common screw. One end of the twine is made fast, and the other is left free to be unwound by the resistance of the air acting upon it during the passage of the cartridge. The unwinding of the twine being completed, the centrifugal force occasioned thereby opens the case and liberates the contents. One of the subordinate points of the construction is, that of filling the interstices of the charge with fine sand, or other matter, either mixed or not with tallow, in order to prevent the fired powder from penetrating into, and deranging the construction of the cartridge; and also for the purpose of preserving the spherical form of the shot, whereby the penetrating force is increased.

The distance to which the shot are to be conveyed in a body is to be regulated by the number of turns, or length of the binding; also by its specific gravity, texture, and size, and by the charge of powder. The shorter the binding, the less will be the distance to which the shot are conveyed in a body, and *vice versâ*. The case should be of the least possible weight to possess sufficient strength to resist any compression by the action of the discharge, and should retain the whole of its contents, until the last turn of the binding is unwound.

The cartridge cases are formed in two halves, which when put

together produce a cylindrical chamber, with hemispherical ends; they are stamped by a punch, so as to have the necessary groove running round each pair; and the two halves, when put together, are tied with a piece of string, or a copper wire, passing through two holes at the extremity of the cartridge, so as to form a hinge, which permits the case to open longitudinally when the string or twine is unwound.—*Trans. Soc. Arts*, xliv. 89.

7. *Elastic Mould for Castings.*—Being much engaged in taking casts from anatomical preparations, Mr. D. Fox found great difficulty, principally with hard bodies, which, when undercut, or having considerable overlaps, did not admit of the removal of moulds of the ordinary kind, except with injury. These difficulties suggested to him the use of elastic moulds, which, giving way as they were withdrawn from complicated parts, would return to their proper shape; and he ultimately succeeded in making such moulds of glue, which not only relieved him from all his difficulties, but were attended with great advantages, in consequence of the small number of pieces into which it was necessary to divide the mould.

The body to be moulded, previously oiled, must be secured one inch above the surface of a board, and then surrounded by a wall of clay, about an inch distant from its sides. The clay must also extend rather higher than the contained body; into this, warm melted glue, as thick as possible, so that it will run, is to be poured, so as to completely cover the body to be moulded; the glue is to remain till cold, when it will have set into an *elastic* mass, just such as is required.

Having removed the clay, the glue is to be cut into as many pieces as may be necessary for its removal, either by a sharp-pointed knife, or by having placed threads in the requisite situations on the body to be moulded, which may be drawn away when the glue is set, so as to cut it in any way that may be required.

The portions of the glue mould having been removed from the original are to be placed together and bound round by tape.

In some instances it is well to run small wood pegs through the portions of glue, so as to keep them exactly in their proper positions. If the mould be of considerable size, it is better to let it be bound with moderate tightness upon a board to prevent it bending whilst in use; having done as above described, the plaster of Paris, as in common casting, is to be poured into the mould and left to set.

In many instances wax may also be cast in glue, if it is not poured in whilst too hot; as the wax cools so rapidly when applied to the cold glue, that the sharpness of the impression is not injured.

Glue has been described as succeeding well where an elastic mould is alone applicable; but many modifications are admissible. When the moulds are not used soon after being made, treacle should be previously mixed with the glue (as employed by printers) to prevent it becoming hard.

The description thus given is with reference to moulding those bodies which cannot be so done by any other than an elastic mould; but glue moulds will be found greatly to facilitate casting in many departments, as a mould may be frequently taken by this method in two or three pieces, which would, on any other principle, require many.—*Trans. Soc. Arts*, xliv. 38.

8. *Leslie's Apparatus for ascertaining the Specific Gravity of Powders**.—Relative to the instrument constructed lately by Mr. Leslie, for the purpose of ascertaining the density of powders, the Bulletin Universelle observes, that it is the same with the Stereometer of M. H. Say, the description of which with figures may be found in the 23rd volume of the Annales de Chimie, (1797,) at page 1. The instrument was made and often used, especially in ascertaining the specific gravity of gunpowder; it still exists in the collection of l'Ecole Polytechnique.—*Bull. Univ.* A. tome vi. p. 361.

9. *Menstruum for Biting-in on Plates of soft Steel.*—This menstruum, the result of the investigations of Mr. Humphrys, consists of a quarter of an ounce of corrosive sublimate powdered, and a quarter of an ounce of alum powdered, dissolved in half a pint of hot water. It must be allowed to cool before being used, and when on the plate, it is to be stirred continually with a camel's-hair brush, and the plate washed perfectly after each biting. As the acid, though clear at first, becomes turbid during its action on the steel, it may be prudent, in fine works, to throw away each portion of acid after it has been on the plate. The taste and experience of the artist must dictate the length of time he may leave it on his plate; delicate tints are obtained in about three minutes. The process of biting is much more difficult of performance on soft, than on hard, steel, perhaps in consequence of the proportion of carbon in the latter, which affects the state of the iron when taken into solution: for, by soft steel, engravers mean not a steel that has been heated and cooled slowly, in contradistinction to one that has been hardened, for they always work upon steel as soft as it can be rendered, without change of composition; but he understands a steel that has been decarbonised, and brought back towards the state of iron. The difficulties which have usually occurred with such partially-decarbonised steel are removed, according to the experience of the artists who have tried Mr. Humphry's menstruum, by its substitution for the solvents previously resorted to.—*Trans. Soc. Arts*, xliv. 53.

10. *Etching on Ivory.*—The usual mode of ornamenting ivory in black is to engrave the pattern or design, and to fill up the cavities thus produced with hard black varnish. The demand for

* See Quarterly Journal, xxi. 374.

ivory thus engraved is considerable. Mr. Cathery has much improved and simplified the process, by covering the ivory with engraver's varnish, and drawing the design with an etching needle; he then pours on a menstruum, composed of 120 grains of fine silver, dissolved in an ounce measure of nitric acid, and diluted with one quart of pure distilled water. After half an hour, more or less, according to the required depth of tint, the liquor is to be poured off, and the surface is to be washed with distilled water, and dried with blotting paper. It is then to be exposed to the light for an hour, after which the varnish may be removed by means of oil of turpentine. The design will now appear permanently impressed on the ivory, and of a black or blackish-brown colour, which will come to its full tint after exposure for a day or two to the light. Varieties of colour may be given by substituting the salt of gold, platina, copper, &c. for the solution of silver.—*Trans. Soc. Arts*, xliv. 57.

11. *Method of Cutting Glass.*—If a tube, a goblet, or other round glass body is to be cut, a line is to be marked with a gun-flint having a sharp angle, an agate, a diamond, or a file, exactly on the place where it is to be cut. A long thread, covered with sulphur, is then to be passed two or three times round the circular line, is to be inflamed, and burnt; when the glass is well heated, some drops of cold water are to be thrown on to it, when the pieces will separate in an exact manner, as if cut with scissors. It is by this means that glasses are cut circularly into thin bands, which may either be separated from, or repose upon, each other, at pleasure, in the manner of a spring.—*Jour. de Connaissances usuelles*, iv. p. 15.

12. *Preservation of Skins.*—I. Stegar, a tanner, at Tyman, in Hungary, uses with great advantage the pyroligneous acid, in preserving skins from putrefaction, and in recovering them when attacked. They are deprived of none of their useful qualities, if covered by means of a brush with the acid, which they absorb very readily.

13. *Indian Method of Varnishing.*—The uncivilised Indians, called Andoquies, who live near the sources of the river Magdalena, in Columbia, South America, use a vegetable substance as a varnish, a specimen of which has been sent to the Society of Arts. When taken from the shrub on which it grows, it is kept in water to preserve it moist: when perfectly dry it is considered useless, as those who work it up have no means of reducing it again to a soft state. When about to be used, it is put into boiling water, to separate from it the oily matter it contains. When effectually freed from the oil, it is taken out, and the varnisher puts a small quantity into his mouth, and continues to chew it as he goes on with his work. After thus chewing a bit of the varnish for some

time, he takes it out, and draws and extends it between his hands till it becomes exceedingly thin, thinner, perhaps, than the finest paper. In this state it is laid on the article to be varnished, and to which it adheres. When used with gold and silver leaf, as is often the case, the gold or silver is placed between two folds of the varnish, and thus applied: the fold on the inside is to make the whole adhere, and that on the outside is to varnish and protect the gold and silver, which shine through, and produce a good effect. The varnish combines readily with various colouring substances, which the people there use with it.—*Trans. Soc. Arts*, xliv. 192.

II. Chemical Science.

1. *Heat evolved from Air by Compression.*—By a mathematical investigation of the heat extricated from air, when it undergoes a given condensation, and by a reference to the experiments of Clement, Gay-Lussac, and others, Mr. Ivory finds, that "*the heat extricated from air, when it undergoes a given condensation, is equal to three-eighths of the diminution of temperature required to produce the same condensation, the pressure being constant.*" Air, under a constant pressure, diminishes 1-480th of its volume*, for every degree of depression on Fahrenheit's scale; and therefore one degree of heat will be extricated from air, when it undergoes a condensation equal to $\frac{1}{480} \times \frac{3}{8} = \frac{1}{1280}$. If a mass of air were suddenly reduced to half its bulk, the heat evolved would be $\frac{1}{2} \div \frac{1}{1280} = 90°$. —*Phil. Mag. N. S.* i. 89.

2. *On the Action of a Moving Metallic Disc, upon a portion of a Voltaic Conductor.*—Immediately after the highly interesting discovery made by M. Arago, relative to the effects produced reciprocally by magnets and metallic plates, when the one or the other were put into a state of motion, M. Ampère was led by his views of the nature of magnetism, to look for a similar effect, when the magnet was removed and replaced by a conducting wire formed into a helix, for when an electric current is sent through such a wire, it is found to possess all the properties of a magnet. A double spiral was formed from wire, and delicately suspended above a horizontal disc of copper, to which a rotary motion was given by convenient apparatus, also of copper. The two extremities of the wire forming the helix were plunged into two circular cups, or troughs, filled with mercury, and placed in communication with the poles of a voltaic battery; a screen was placed between the copper-plate and the suspended spiral. Immediately upon moving the plate, the spiral began to turn in the same direction, precisely like a magnetic bar, or like the assemblage of two vertical magnets attached to a horizontal lever. Ultimately the rotary movement became accelerated and continuous. Upon altering the direction

* At 32° F.—Ed.

of the motion in the disc, the motion of the spiral immediately changed, in all cases continuing the same as that of the plate. This satisfactory fact is a powerful addition to those by which M. Ampère supports his views of the nature of magnetism generally, and its origin in all cases from currents of electricity.—*Bull. Phi.* 1826, p. 134.

3. *Influence of the Aurora Borealis on the Magnetic Needle.*—A luminous arc produced by an aurora borealis was seen on the 29th of April, 1826, at Carlisle and in Roxburghshire. This phenomenon was not seen at Gosport, though the sky was very clear. At 7 h. 50′ P.M. of the 29th April at Paris, the north point of the needle indicating diurnal variations was 4′ to the east of its ordinary position; at half past eight o'clock it passed westward with a ready movement; at three-quarters past eleven o'clock it had returned within half a minute to the position it had at half-past eight. Long experience has taught me that these great oscillations, at hours when the needle is generally stationary, is an indication almost certain of the existence of an aurora borealis. M. Arago, the author of these observations, then ventures to announce, solely from the effects he has observed upon the magnetic needle, that brilliant aurora boreales will have been observed in some part in the north on the following days of the year 1826: the 16th of January, the 10th and 13th of February, the day of the 9th of March, the morning and evening of the 23rd of March, and the 29th of the same month, the 9th and 13th of April, the nights of the 17th and 18th of April, the 24th of the same month, &c.—*Annales de Chimie*, xxxiii. 421.

4. *Electro-chemical Appearances.*—M. Nobili has been induced to contract one of the poles of the voltaic pile in the manner first pointed out by Dr. Wollaston, namely, by inclosing a wire within a glass tube, and exposing only a sectional surface, and has observed the effects which occur when such a pole is used in the decomposition of various saline solutions, the other pole being a plate of metal, brought very near to it. The general results appear to be two in number. The first consists in the property remarked of certain electro-negative substances to attach themselves, in certain determinate circumstances, to the surface of those metals which are least oxidable, in thin regular layers, so as to present the beautiful phenomenon of coloured rings in a splendid manner. This effect, it is supposed, may be applicable in the ornamental arts. The *second* is, that when electro-negative substances are not deposited in thin plates upon the metals, they generally attack their surfaces, not in an uniform manner, as might be supposed, but by a gradation of intensity from the centre, continued and decreasing, but at irregular intervals, following, as it were, a law analogous to that of *interferences*. The same phenomenon is observed in the attachment of electro-positive substances to the negative pole, namely, an alternation in the circles of oxide and of metal. M. Nobili then

inquires, whether it can be true that radiation of the electric currents is subject to a law of interference, and acknowledges, notwithstanding the alternations he has obtained, that new experiments are required.—*Bib. Univ.* xxxiii. 302.

5. *Earthy Depositions from Water influenced by the Electricity of Metals in contact.*—M. Dumas has noticed an interesting case of voltaic action, which has occurred with water contained in metallic tanks and tubes. The water from the springs in the neighbourhood of the Seine are strongly charged with carbonate of lime, held in solution by carbonic acid. When it is received and retained as before mentioned, it occasions incrustations, which, however, are not indifferent as to the places on which they occur, but are governed by the electrical relations of the metals forming the vessels. The Sévres porcelain works are supplied with water of this kind, which is received and retained in a leaden reservoir; the general internal surface of the lead presents scarcely a trace of deposit, but a thick coat has been formed on the lines of solder by which the plates of lead are joined. The incrustation is irregular, but evidently crystalline; it is slightly coloured by oxide of iron, and dissolves entirely, with effervescence, in nitric acid. A bar of iron used to raise a valve, and consequently plunged in the water, was covered with a layer five or six lines in thickness, the lead in the immediate neighbourhood being nearly free from the deposit.

In the tubes which conduct the water, the deposit occurs only at the soldered joints. The plumbers know this fact, and when the pipes appear to be stopped up, always go to these places. The copper cocks are also the localities of deposition.

Abundant proof was obtained that these effects were voltaic, by immersing simple combinations of metals into portions of the water; and M. Dumas proposes a remedy for the evil occasioned by the deposition, by causing it to take place in unimportant or accessible situations. For this purpose he considers the assemblage of tubes as an immense plate, and proposes to attach lateral tubes to it, at intervals from twenty to thirty feet; these are to be closed by metallic plugs, containing rods affixed to them projecting into the water, of such a nature as to become the negative part of the arrangement; all the depositions will then take place on these stoppers and rods, and none upon the lead. From the effects observed, it is concluded that cast-iron will well answer the purpose.—*Annales de Chimie*, xxxiii. 265.

6. *Instance of a returning Stroke by Lightning.*—The returning stroke of lightning is well known to be due to the restoration of the natural electric state, after it has been disturbed by induction. Thus if a person be brought into a highly electric and negative state by induction, from the approximation of a body highly charged positively, and then the latter be discharged by means having no connexion with the negatively electrified person, the negative state

of the latter will be immediately destroyed, and an effect, in part analogous to that of a positive discharge of electricity, will be produced. Some of the most serious accidents which occur from lightning are supposed to be produced in this way, not by the mere disturbance of electricity in a person only, but of the electricity of those bodies with which the person may be in contact, and to which he accidentally serves as a conductor.

On the 24th of Sept. 1826, at the moment when the lightning struck the ground, at the farm of Gali, near Versailles, M. B—— was violently affected by a returning stroke, at the distance of half a league from the place. The following are the circumstances of the case :—A violent storm occurred at Versailles and the neighbouring parts, at half past nine o'clock. M. B., aged seventy-two years, was passing the Rue Dauphine, at a little distance from the church of Notre Dame, when one of those whirlwinds so common in the neighbourhood of large buildings, obliged him to turn round. He was then close to the party-wall of the houses 13 and 14, his right side being at a small distance from it. A metal water-pipe was fixed up the front of the house in this place, bringing the rain from the roof to the level of the pavement. In this position M. B. felt a commotion, which he describes as if all the right side of his body was roughly thrown towards the left, feeling, at the same time, much oppression, and vertigo, resembling that of drunkenness. The immediate effects were, difficulty of motion in all the left side, and a disturbed respiration ; and it was with much difficulty, and only by resting frequently, that M. B. could reach the house of a neighbouring friend. It was there observed, that the tongue was embarrassed in its motions as well as the left side, but by the aid of attention the agitation of the mind was calmed ; the night passed moderately, and the next morning all was nearly in its ordinary state. In the evening, however, at the hour when the circumstance occurred, all the symptoms returned, and the same results occurred daily until the end of the week, when a physician was consulted. He immediately recognised the symptoms of compression on the brain and spinal-marrow, from which had resulted an incomplete paralysis of the tongue and the left arm and leg. This speedily gave way under the hands of the physician, but the periodical returns occurred until the cure was completed.

It would be difficult to prove the identity of the electric discharge which fired the farm of Gali, and struck M. B., but the latter cannot be attributed to a direct stroke ; for at the moment when it happened, the intervals between the lightning and thunder were such as to shew that the storm was not over Versailles. By a coincidence of circumstances, M. Demonferrand, who describes the case, was in the house No. 15, the whole of the evening, in an apartment contiguous to the metal-pipe which appears to have served as a conductor for the electricity, but neither he nor any other person in the house felt the slightest disturbance. In the opposite house was a person in a bad state of health, and therefore, perhaps,

more sensible to electric changes, but neither did he experience any change in his feelings at the moment.—*Annales de Chimie*, xxxiii. 417.

7. *Phosphorescence of Fluate of Lime.*—Fluate of lime, though a highly phosphorescent substance, requires either to be heated or rubbed, for the emission of the light. A fluate of lime obtained from Odonschelon, in Daourie, was, however, found to be luminous without this preparation, the light being of such intensity as to render the piece visible without being placed in perfect obscurity. Its luminous state has continued ever since, and nothing indicates that it will cease. The intensity of the light is not always the same, and appears to be changed by changes in the atmosphere; it is greatest in wet and stormy weather. The contact of air appears to exert an influence on the light, for pieces of the fluor closed in a drawer for eight days, were found scarcely luminous, but being exposed to air became equally brilliant as at first. When plunged into water the degree of phosphorescence appeared to be much more constant. The light was of a feeble yellowish-green colour; a piece of the substance two inches in diameter gave light sufficient to permit words to be read at some lines distance. According to the observations of M. Becquerel, heat has great influence over the phosphorescence: at zero the light is scarcely visible; plunged into boiling water, it became very luminous; upon mercury heated to 300° *C.*(572°F.), the light was so strong as to allow printed characters to be read at seven inches and a half distance. This high temperature injured the phosphorescent power of the substance at common temperatures for a short time; but it was restored to its full intensity by exposure for a few hours to solar light: finally, it appeared that atmospheric causes, not as yet understood, exert a great influence upon the substance.

The object of the note is to point out the curious property possessed by this fluate of lime, of remaining permanently luminous. It is found in a primitive granitic rock, in company with beryl, topaz, wolfram, &c., and differs, in appearance, from the ordinary varities of fluor spar, by its peculiar waxy or resinous lustre. It is found massive, having a structure more interlaced than the ordinary varieties, and sometimes crystallised; it then has very complicated forms.—*Bull. Philomat.* 1826, 104.

8. *Chemical Compounds produced by physical Powers.*—"Sitting of the Royal Academy of Sciences at Paris, 11th December, 1826. M. Becquerel announced, that he had succeeded in producing new compounds, by means of physical powers only. Each of the new compounds has a peculiar crystalline system, and most of them have the property of being decomposed by water."—*Annales de Chimie*, xxxiii. 439.

9. *Table of the Density of Vapours and Gases*, examined by M.

Dumas.—This table gives the specific gravity of vapours and gases, as compared to air, and also the density, as calculated from the weights of their elements, and from certain experiments. There is likewise in the table a column indicating the weight of a litre of each gas, or vapour, at the temperature of 32° F., and barometric pressure of 29.92 inches, the weight being expressed in grammes.

	Observed.	Calculated.	Weight.
Iodine	8.716	8.6118	11.3230
Mercury	6.976	6.9783	9.0625
Proto-chloride of phosphorus	4.875	4.8076	6.3532
Arsenuretted hydrogen	2.695	2.6950	3.5023
Proto-chloride of arsenic	6.3006	6.2969	8.1852
Chloride of silicium	5.939	5.9599	7.7154
Fluo-silicic acid	3.600	3.5973	..
Chloride of boron	3.942	4.0793	5.1212
Fluo-boric acid	2.3124	2.3075	..
Per-chloride of tin	9.1997	8.9930	11.9514
Per-chloride of titanium	6.856	7.0470	8.881
Phosphorus	..	2.2052	..
Arsenic	..	5.1839	..
Silicium	..	1.0197	..
Boron	..	0.7487	..
Tin	..	4.0530	..
Titanium	..	2.1070	..

Annales de Chimie, xxxiii. 391.

The litre is equal to 61.02525 cubic inches; the gramme to 15.44 grains.

10. *On the Existence and Nature of Bromine.*—Professor Liebig has repeated the experiments of M. Balard, relative to the production of this peculiar body upon the mother waters of the salt-works of Theodorshalle, near Kreutznach, and has obtained as much as 20 grammes (308.8 grains) of bromine from 30 pounds of the fluid. The process adopted was that of M. Balard. After repeating most of the experiments of M. Balard, with consistent and satisfactory results, M. Liebeg proceeded to some which he considers as supporting the simple view of the nature of the substance. Iron was heated to redness in a glass tube, and the vapour of bromine dried by chloride of calcium, passed over it; on coming into contact with the metal, the latter became incandescent, without any disengagement of gaseous matter; the mass fused into a body of a clear yellow colour, having, when cold, a lamellar structure, and dissolving readily in water. It was proto-bromide of iron, and yielded bromine on the action of chlorine. Platina and lamp-black did not act on the vapours of bromine: 2.521 parts of the bromide of potassium being decomposed by nitrate of silver, gave 4.041 parts of bromide of silver, which, by calculation, gives 94.11 as the atom of bromine oxygen being 10.—*Annales de Chimie*, xxxiii. 331.

Royal Academy of Sciences at Paris, Oct. 2,—M. Chevruel

announced to the academy that M. Dumas had discovered a chloride of iodine, having all the properties of the *bromine* recently described as a simple body by M. Balard.—*Bull. Univ.*, c. ix. 277.

11. *Preparation of Chloride of Boron.*—M. Dumas obtains chloride of boron by passing dry chlorine over an incandescent mixture of charcoal and boracic acid. It is a gaseous body, which may be collected over mercury, and corresponds in its composition with fluo-boric acid. The apparatus used consisted of a porcelain tube, containing the mixture. The dry chlorine was introduced at one extremity, the other was attached to an adopter, and that to a bent tube, plunged into mercury. The tube was first heated for some time, to expel all humidity, then the gas introduced, and when it had continued to pass for about a quarter of an hour, the adopter and bent tube were attached, and the gas collected. When the small tube became obstructed by flocculi resulting from the action of water, it was replaced by another.—*Ann. de Chimie*, xxxiii. 378.

12. *Chloride of Boron.*—M. Despretz lays claim to the discovery of the chloride of boron, which, according to a letter to the Editor of the Annales de Chimie, he announced to the Société Philomatique, some years ago. He obtained this compound by passing dry chlorine, at a high temperature, either over boruret of iron or a mixture of charcoal with boracic acid or borax. In the first case, the chloride of boron is mixed with a certain quantity of chlorine, which may, however, be absorbed by mercury: in the second case, the gas collected contains oxide of carbon, carbonic acid, and muriatic acid. The apparatus consists of a balloon, in which the chlorine is to be generated; a porcelain tube, containing the mixture; an adopter destined for the condensation of the chloride of iron or boracic acid disengaged during the operation; and finally, a tube, the end of which dips into mercury. The gas collected by either of the processes is colourless, denser than the air, decomposable by water, fuming in contact with air, resisting a high temperature, &c.—*Ann. de Chimie*, xxxiii. 442.

13. *Preparation of Chloride of Arsenic.*—One part of arsenious acid, with ten parts of concentrated sulphuric acid, is to be put into a tubulated retort, and the temperature raised to nearly 212° F. Fragments of fused common salt are then to be thrown in by the tubular. By continuing the heat, and successively adding common salt, proto-chloride of arsenic is obtained; it falls drop by drop from the beak of the retort, and may be collected in cooled vessels. Little, if any, muriatic acid is disengaged; but towards the end of the operation a portion of hydrated chloride of arsenic is frequently produced, which collects in the vessel, above the pure chloride. The two bodies do not mix; the hydrate is liquid, transparent, and colourless, and more viscid than the

dry chloride. The hydrate may be decomposed, and a pure chloride obtained, by distilling the mixture from a sufficient quantity of concentrated sulphuric acid.—*Dumas, Ann. de Chimie,* xxxiii. 360.

14. *Preparation of Oxide of Chrome, as a Pigment.*—M. Nasse recommends that a chromate of mercury should be formed, according to the usual process, by precipitation from a chromate of potash and a nitrate of mercury, as neutral and concentrated as possible. The oxide of chrome, obtained by heating the chromate of mercury, will have the greatest perfection of colour, if it be put into an unglazed porcelain crucible, and exposed to the heat of the furnace during the time required for baking the porcelain. The oxide produced will have a fine grass-green colour.

The following directions are given for the preparation of the blue oxide: the concentrated alkaline solution of chrome is to be saturated with weak sulphuric acid, and then to every 8lbs. is to be added 1lb. of common salt and half a pound of concentrated sulphuric acid; the liquid will then acquire a green colour. To be certain that the yellow colour is totally destroyed, a small quantity of the liquor is to have potash added to it, and filtered; if the fluid is still yellow, a fresh portion of salt and of sulphuric acid is to be added: the fluid is then to be evaporated to dryness, re-dissolved and filtered; the oxide of chrome is then to be precipitated by caustic potash. It will be of a greenish-blue colour, and being washed, must be collected upon a filter.—*Bull. Univ.* E., vi. 275.

15. *Separation of Iron from Manganese.*—The novelty of situation in which the chemical analyst is frequently placed, in pursuing his intricate experimental inquiries, often renders one process for the separation of substances useful, when another of a generally superior character is inadmissible. It is for this reason that we insert at present a mode of separating iron from manganese, proposed by M. Quesnevile, jun.:—Dissolve both oxides in muriatic acid, and boil the solution for some time, to expel all excess of acid, in order to render the solution as neutral as possible. Dilute the solution with a large quantity of water, and pass chlorine gas through it, to peroxidize the iron entirely; then precipitate the liquor by arseniate of potash: the precipitate is of a greenish-white colour, and consists entirely of arseniate of iron. After some hours, filter the liquor, and wash the precipitate with a large quantity of boiling water; dry it, and calcine it strongly, to obtain the oxide of iron; evaporate the solution, which contains the arseniate of manganese, almost to dryness, and add water to it; if there remain by accident any traces of arseniate of iron, it separates: then filter and decompose the solution by caustic potash, and the oxide of manganese, when well washed, is then perfectly pure.—*Jour. de Phar.*, Sept., 1826.

16. *Pure Proto-Salt of Iron.*—A solution of protoxide of iron, without any admixture of peroxide, may be obtained by putting iron into an aqueous solution of sulphurous acid, and suffering the mixture to remain for a short time without contact of the air. When a solution of the ferro-cyanate of potash is added, a perfectly white precipitate is formed, which is the proto-ferrocyanate of iron.—*Phillips, Phil. Mag., N. S.*, i. 72.

17. *Action of Potassium on Oxalates.*—When well-dried oxalate of lead, mixed with very small portions of potassium perfectly freed from naphtha, is heated in the bottom of a glass tube, air being carefully excluded by excess of the oxalate, a violent detonation suddenly takes place, before the heat is sufficiently great to effect the decomposition of the oxalate, when no potassium is present. The tube is spotted with metallic lead, the potassium is oxidized, and there is no carbon deposited. An examination of the gas resulting from this instantaneous decomposition may elucidate the nature of the oxalates, but hitherto the apparatus employed has always been broken by the explosion. Oxalate of copper, treated in the same way, has always occasioned strong detonations, and metallic copper appears.—*Jour. de Phar., Phil. Mag. N. S.*, i. 145.

18. *Analysis of the Triple Prussiate of Potash.*—Mr. Phillips, by carefully experimenting with the crystallized triple prussiate of potash, obtained the following proportions per cent. of three of its elements :—

Iron	13.58
Potassium	36.75
Water	12.50

the remaining 37.17 parts are assumed, according to the analysis of Berzelius, to be cyanogen. The nature and composition of the salt, which he considers as most probable, is as follows :—

6 atoms	carbon	36
3 „	azote	42
3 „	hydrogen	3
1 atom	oxygen	8
1 „	iron	28
		117 = 1 atom ferro-cyanic acid
2 atoms	potash	96
		213

Viewing this as the constitution of the salt, it is a di-ferrocyanate of potash, or composed of one atom of sand and two atoms of base.—*Phil. Mag. N. S.*, i. 110.

19. *Preparation of Medicinal Chloride of Soda.*—Mr. Payen has published a process, by which he prepares this substance of

uniform strength, without involving the trouble of first preparing chlorine gas. For this purpose he has recourse to the mutual action of carbonate of soda and chloride of lime. M. Payen first ascertained the mean proportion of chlorine in the medicinal solutions of M. Labarraque, and then compared it with the proportion of chlorine in chloride of lime, represented by 98 degrees of Gay-Lussac's chlorometer. These experiments showed that 1 part by weight of chloride of lime of 98° was equivalent to 20 parts of medicinal chloride of soda; that 100 parts of this chloride of lime are completely decomposed by 138 parts of crystallized sub-carbonate of soda; and finally, that, by adding 62 parts of the last salt to the neutral solution of the chloride, its excess was sufficient to render the compound permanent. The following is therefore the formula for the preparation of the chloride of soda:—

Chloride of lime at 98°	100	by weight
Crystallized sub-carbonate of soda	138	„
Water	1800	„

The chloride of lime is to be dissolved, and the sediment well washed; carbonate of soda dissolved by heat is to be poured into the solution, the precipitate allowed to subside, the clear fluid decanted, and the solid matter washed on a filter. The collected solutions are neutral chloride of soda; 62 parts of the sub-carbonate of soda are then to be dissolved in the remainder of the water, and added to the preparation; the whole being then filtered, a limpid solution is obtained, indicating 5° by the areometer of Baumé. It is the pure chloride of soda.

If the chloride of lime be of any other strength than 98°, then the relation is easily ascertained by the chlorometer and a brief calculation, and the equivalent of the quantity above directed must be used.—*Bull. Philom.*, 1826, 151.

20. *Mode of Action of Disinfecting Chlorides.*—The combinations of chlorine with lime and with the carbonate of soda, have lately been highly recommended as disinfecting agents, and apparently with some reason. The names which have been given to the substances, and the statement of their effects and mode of action have, however, been singularly irregular, vague, and uncertain; and it is, therefore, with some pleasure we refer to certain experiments and observations of a clear and satisfactory nature, by M. Gaultier de Claubry.

Well-saturated chloride of lime, being dissolved in water, was submitted to the action of a current of carbonic acid gas; chlorine was quickly disengaged, and by continuing the operation a sufficient length of time the whole was expelled; the liquor no longer possessed bleaching powers, and carbonate of lime had been formed and precipitated, except a small portion dissolved by the excess of carbonic acid.

Air passed slowly through a saturated solution of caustic potash, was then made to traverse a solution of the chloride of lime: no chlorine was liberated; no effect of any kind produced.

The carbonate of lime resulting from the decomposition of the chloride of lime by carbonic acid, retains no chlorine in combination.

The chloride of soda is equally decomposed by carbonic acid, only more slowly, in consequence of the solubility of the salt produced. A specimen of chloride of lime, being examined as to the quantity of muriate that it might contain, was found to have exactly the same quantity before and after decomposition by carbonic acid gas.

A filtered solution of the chloride of lime was exposed to air on the 13th of August; on the 10th of October it no longer contained chlorine, did not affect litmus, and the precipitate from it proved to be carbonate of lime. A stronger solution of the same chloride, exposed to air, on the 16th of August, was found in the same state as the last on the 10th of October.

Notwithstanding that these experiments sufficiently indicated the manner in which these compounds acted on the putrid miasmata floating in foul air, some further experiments were made in the following manner:—Air was passed through blood, which had been left to putrefy for eight days; being then passed through a solution of the chloride of lime, carbonate of lime was deposited, and the air was rendered inodorous and completely purified. In a second similar experiment, the fetid air was passed through a saturated solution of caustic potash, before it arrived at the solution of chloride; the latter then had no effect upon it, and the air retained its insupportable odour. Another experiment was made with air left for twenty hours over putrescent blood; the portion of it, which was passed directly through the chloride, was perfectly purified, but when previously freed from carbonic acid, the chloride had no effect upon it.

It is easy to explain the preference given in many cases to the application of chlorides, as disinfecting agents, above fumigation by chlorine. The carbonic acid in the air, arising from the decomposition of animal matter, liberates the chlorine from its combination; and as this action is slow, the chlorine is less susceptible of affecting the animal economy, but readily decomposes putrid miasmata. It is, therefore, a true fumigation by chlorine, only less violent than those made directly by the gas, and continuing for a longer time.—*Annales de Chimie*, xxxiii. 171.

21. *Deleterious Effects of Carbonic Acid Gas.*—The conclusions to a memoir, by M. Collard, on this subject, are as follows:—i. Animals plunged into carbonic acid gas do not die, as in hydrogen or nitrogen, from the want of air only. ii. That the experiments by which Bichat, Nysten, and other modern authors, have endeavoured to establish a contrary statement, are inexact in numerous points. iii. That carbonic acid gas exerts a special deleterious action.

iv. That this action principally affects the brain and nervous system. v. That the best remedial process for accidents occasioned by carbonic acid gas, consists, probably, in excitations of the diaphragm produced by the voltaic pile.—*Bull. Univ.* C. ix. 321.

22. *New Acids obtained from Animal Substances by Alkalies.*—MM. Chevreul and Gay-Lussac, by treating animal substances with alkalies, have obtained various acids remarkable for their property of neutralizing bases, and in which nitrogen enters as an element. They are occupied, at present, with these acids, and will publish their results as soon as they are completed.—*Annales de Chimie*, xxxiii. 335.

23. *Sugar of Melons.*—The melon has occasionally been thought of as a source of sugar, and its juice submitted to processes calculated to separate that principle from the accompanying but deteriorating matters. M. Payen has lately applied analytical processes to the juice of a melon cultivated near Paris, and from 100 parts of the liquid obtained 1½ parts of sugar, perfectly well crystallized in rhomboidal parallelopipeds, and having the taste and other properties of cane-sugar, with which it appeared to be identical.—*Bull. Philom.* 1826, p. 135.

24. *Method of Destroying the Empyreumatic Odour of Alcohol*, by Dr. Witting.—The purification of alcohol by chloride of lime is not expensive. The action is chemical, and analogous to that of bleaching; the empyreumatic parts are entirely destroyed. The following is the manner in which it has been employed: two ounces of the chloride were mixed with spirit into a uniform clear fluid, which was then put into a distillatory apparatus, with 150 measures of spirit; all the joints were then luted, and the distillation commenced. The first measure of product had a slight odour of chlorine, and was preserved apart for rectification; the rest of the produce was perfectly pure. The chloride made use of should, when dissolved in 26 parts of water, bleach vegetable colour with which it may be mixed.—*Bull. Univ.* E. vi. 333.

25. *On the Colouring Matter of the Madder*, by MM. Robiquet and Colin.—Diffuse the ground madder in three or four parts of water, submit the whole to strong pressure, and repeat the washing thrice; macerate the residue in a water-bath in five or six parts of water, containing half a pint of alum; filter, and precipitate the fluid by sub-carbonate of soda. The precipitate should be carefully washed; the macerations are to be repeated in alum-water until the colour is exhausted. By this means a result is obtained in three hours, which otherwise would require months, and everything induces the supposition that the diminution in price of madder lakes, resulting from such simple methods, will permit their employment even on stained papers.—*Globe, Nov.* 11.

III. Natural History.

1. *Subterraneous Noises heard at Nakous.*—Nakous is situated three leagues from Tor, on the Red Sea. This place is celebrated because of certain sounds produced there at all hours of the day and night. When Mr. Gray, of Oxford, visited Nakous, he heard from beneath his feet a continued dull noise, which soon changed into pulsations, more and more intense, similar to the beats of a time-piece. The next day, Mr. Gray heard the sounds during a whole hour, the weather being clear and serene; and he concluded that the effect could not be produced by the passage of air through certain crevices in the soil and rock, every examination for such fissures in the soil or rock having failed.

The following account of the same place is by M. Seetzen, and was published in 1812:—There is near Tor, a mountain, not only the most remarkable in certain respects of any in Arabia Petræa, but in the whole world. It is called *el Nakous*, and is three leagues north of Tor: two years ago I had heard it spoken of by the Greeks, first, of the convent of Sinai, afterwards, at Suez; but the description being accompanied with fabulous recitals, induced me to suppose the whole a monkish invention. When I gained more information at *Wady el Nachel*, not only were these first accounts confirmed, but new prodigies added.

M. Seetzen then goes on to state, that, "accompanied by a Christian Greek and some Bedouins, I departed for the mountain on the 17th of June, at five o'clock in the morning: after a quarter of an hour's progress, we arrived at the foot of a majestic rock of sandstone; the mountain, perfectly naked, was entirely composed of it. I found many Greek and Arabic names on the rock, and some even in Coptic characters, proving that the place had been visited for ages. At mid-day we arrived at the part of the mountain called *Nakous*: here, at the foot of the chain, we found an insulated rock. On two sides the mountain presented two surfaces, so highly inclined that the white and slightly-adhering sand which covered them was scarcely supported, and slided at the slightest vibration, or when the burning rays of the sun destroyed their slight adherence. These two sandy surfaces were about 150 feet in height; they re-united behind the insulated rock, forming a sharp angle, and, as well as the adjacent surfaces, were mingled with steep rocks, most frequently formed of a friable white sandstone.

"The first sound was heard at one hour and a quarter after mid-day. We climbed with difficulty up the sand-bank, a height of 70 or 80 feet, and stopped under certain rocks, at which it is the custom to listen. Whilst creeping, I heard the sound from under my knees, which induced me to suppose that the slipping of the sand was the cause, and not the effect of the sonorous vibration. At three o'clock, the sound was again heard more strongly, and continued for six minutes; then ceasing for about ten minutes,

it was again produced. It appeared to me like that of a humming-top, and increased and diminished, like the sounds of an Æolian harp. To convince myself of the truth of my conjecture, I crawled with the utmost difficulty up to the highest rocks, and then allowed myself to slide as rapidly as possible, endeavouring with my feet and hands to put the sand in motion. The effect thus produced was so great, and the sand rolling beneath me caused a sound so intense, that the earth appeared to tremble, and I should have been dismayed if the cause had been unknown to me.

"But how can this motion of the sand produce so intense an effect, and one observed, I believe, no where else? Does the rolling layer of sand act like a bow, which by its friction on a plate of glass will cause the powder spread over it to tremble and acquire determinate figures? Does the fixed and adhering layer of sand perform the part of the glass plate, and the neighbouring rocks that of the resounding body? These are questions which the natural philosopher must decide.—*Ann. de Chimie*, xxxiii. 435.

2. *Italian Cavern containing Bones.*—With the exception of a small cavern in the island of Elba, none containing bones like those which have become so celebrated in England and Germany, had been discovered in Italy. Lately, however, Professor Gaetano Savi has discovered one in one of the mountains which surround the gulf of Spezia, opposite to Cassana, a village not far removed from Casale. Many of the bones contained in this cavern belong to the same species of bear of which the bones have been found in such large quantities in the caverns of northern Europe. Other bones belong to the ruminantia, others to herbivorous animals, and apparently to the stag. All these bones are broken and incrusted.—*Giornale de Fisica*, ix. 154.

3. *Crystallization of Asbestus.*—M. Hess, of Dorpat, has examined a mineral from the village of Pitkaranda, near the lake Ladoga, and has determined it to be a crystallized variety of asbestus. He describes its primitive form as an oblique rhomboidal prism, of 84° 36′, of which the base is inclined on the obtuse sides, with angles of 104°. Its analysis gave him, silica, 45.57; protoxide of iron, 19.73; alumine, 3; lime, 4.4; magnesia, 23.4; water, 2.—*Archives des S. Nat.* v. 321.

4. *Dense Fogs.*—There have been remarked at different times in the winter season such dense fogs as to occasion serious accidents from their interference with distinct vision. These are observed in and about large towns only, and are supposed by M. Defranc to originate from the stagnation of the smoke and vapours generated in such situations. On certain calm winter days, it may be observed that the smoke, on leaving the chimneys,

falls to the ground; and that downward currents set through those flues, at the bottom of which there is no fire; and it is supposed that when this takes place in a town, it occasions the fogs peculiar to those accumulations of houses. Many instances are then quoted, according with these views, but M. Arago objects to the conclusion, remarking, that, in the first place, these fogs often form in a few minutes; and in the next, that they frequently do not occur in the calmest, and according to M. Defranc's supposition, most favourable, weather.—*Ann. de Chimie*, xxxiii. 415.

5. *Atmospheric Phenomenon; Ohio, U. S.*—The following effect is described by Mr. Atwater: "Before a storm here I have often noticed in an evening of the latter part of Autumn, and sometimes in the winter, a phenomenon not recollected by me to have been seen on the east side of the Alleganies. Some one spot or spots near the horizon, in a cloudy night, appeared so lighted up, that the common people believed there was some great fire in the direction from which the light came. I have seen at once two or three of these luminous spots not far from each other; generally, there is but one, and a storm, invariably proceeding from the same point near the horizon, succeeds in a few hours.—*Silliman's Journal*, xi. 226.

6. *Effects of Cold.*—The following natural temperature, and its effects, which occurred at Marietta, in the state of Ohio, are recorded by Dr. Hildreth, of that place. Feb. 9th, 1818, at 7 A.M., Fahrenheit's thermometer sunk to 20° below zero. On the 10th, at half-past 6 A. M., it was down to 22° below zero, in the open air, and suspended to the limb of a tree. With all this severity of cold, the Ohio river was not frozen across; it was full of floating ice, and during the night and morning of the 9th and 10th, threw up a continual cloud of vapour, which darkened the air; and freezing as it ascended, fell again in a moderate shower of snow. So intense was the cold, that there was a continual cracking and snapping, by the contraction of wood in buildings and in trees. Very strong brandy, exposed in a tea-saucer to the open air, through the night of the 9th, was found frozen to ice the next morning. The weather was colder at this time, by ten or twelve degrees, than has been known since the country was first inhabited." —*Silliman's Journal*, xi. 232.

7. *Notice of Mr. Burchell's Travels in South America.*—It will be gratifying to our scientific readers to know that, notwithstanding the liberal encouragement given by the continental governments to researches in South America, some portion of the harvest will be reaped by this country, by the enterprise and unassisted exertions of her scientific sons. Mr. Swainson explored the provinces on the north-east coast of that vast empire a few years ago. But a far more interesting and hazardous journey is now performing by Mr.

Burchell, the well-known African traveller. It is not generally known that this learned naturalist accompanied Sir Charles Stuart, on his diplomatic mission to Rio de Janeiro, two years ago; carrying with him, among other things, a collection of astronomical and mathematical instruments by the first makers, with an intention of using them on the Cordilleras of Peru and Paraguay. Mr. Burchell remained some time at Rio, to become seasoned to the climate, and to make preparations for his arduous undertaking. In a recent letter, dated at that place on the 7th September last, received by Mr. Swainson, he thus sketches out his route:—

"I have made large collections here, and in the neighbouring country, and shall embark to-morrow morning for Santos, and travel thence to St. Paul's, where (or at Jundiahy) I shall purchase about twenty mules, to carry my baggage, consisting of thirty-three packages, mostly weighing a hundred pounds each; these will be accompanied by a proportionate number of muleteers and attendants. My plan is to traverse Brazil; and passing through Matto Grosso (*i. e.* the Great Wood,) the central province, enter Peru, and visit the city of Cusco, the lake Titicaca, and the various ruins of the times of the Incas. I am not certain whether I shall go so far northward as Lima; but if nothing untoward occurs to derange my plans, I shall go to Arequipa, Potosi, Solta, &c. &c., and thence to Buenos Ayres, where my South American journey will end."

Looking upon Mr. Burchell as the most scientific and accomplished traveller, next to the celebrated Humboldt, that this age has produced, we anxiously look forward to his success and return, as to a period when every branch of science will derive advantage from his accuracy and research.

8. *Note on the Motions and Actions of Quadrupeds.*—It is asserted by physiologists that the right side of animals is generally stronger than the left; and it is said by riding-masters that horses most naturally gallop with the left foot foremost, which they attribute to the foal being commonly placed, before birth, with its head to the left. All these facts, however, must be admitted with some hesitation: the first thing to be asked is, what is meant by galloping with the right or left foot foremost; and we shall find that the foremost foot is always most bent, and is put down singly, and then, after some pause, the foot diagonally opposite, and then, after a shorter pause, the two others almost at the same instant; these two remaining parallel to each other, and appearing to be the most energetically employed in the progressive motion. So that, in fact, when the horse gallops with the left foot foremost, the principal effort seems to be made by the right foot before, and the left foot behind, and neither the right nor left side of the body in general can be said to be called into action more than the other.

It is curious to observe the *association of ideas*, or of *impressions*, by which the actions of horses are sometimes regulated. A young horse, which had been very *well* taught to go in the usual manner with

the right leg foremost, could not be persuaded by any art to depart from his instructions; till, after vain efforts for several months to make his paces more classical, his rider recollected that he had once or twice slightly started at a particular spot in the road, and gone off with the left foot; he determined to make trial of the association with this spot, and having succeeded almost at once in the experiment, he put out his left hand to caress the animal's neck; and this motion was soon understood as a signal for the same pace, so that by thrusting forwards the left hand, the object was attained in any other part of the road, until the leg was substituted for the arm, in the regular manner.

9. *Measurements and Weight of a Sea-gull.*—These measurements being taken with reference to the means by which a bird sustains itself in the air do not profess to be particularly accurate. The object was to ascertain generally the proportion of surface exposed to the air, and the weight it had to sustain.

	f.	in.
Between the tips of the wings	4	10
From the beak to the end of the tail	2	4
Estimated average width of the wings through the 4 f. 10 in.	0	7½
From the end of the tail to the setting on of the wings	0	9
Width of the tail part near the wings, *i. e.* of the body there	0	5
Width of the extremity of the tail when moderately extended	0	9
Length of each wing	2	3
Thickness of the body	0	5

The weight of the bird, with all its feathers, was 3 lb. 4½ ounces, or 52½ ounces avoirdupois.

10. *Excision of Teeth.*—We copy the following remarks from Mr. Fay's communication on the subject to the Society of Arts:—"In the centre of every tooth is a little cavity, in which is expanded a nervous pulp, forming the principal seat of sensation in that organ; the nervous twigs pass through the roots of the teeth by very minute passages into this cavity, where they are spread out. The base of this cavity is situated a little above the level of the neck of the tooth, and it struck me that when caries had extended so far as to expose this nervous matter to the various agencies, from which, in the healthy state of the parts, it is protected, it would be easy to remove that part of the tooth containing the cavity, the seat of the pain, and thus allow the sound roots to remain in their sockets undisturbed, to support the adjoining teeth; as it is a notorious fact that the entire removal of one tooth, however easily performed, causes the adjoining ones to become prematurely loose, and ultimately to fall out, in consequence principally of the absorption of the alveolar process. Minutely to describe how this happens would occupy too much

space, and I mention it merely as a well-known fact, to show the value of allowing the root or fangs of a tooth to remain. This operation I have called the *operation of excision*, and I recommend it as a most valuable substitute for the extraction of the teeth in the majority of cases of caries; but by no means to supersede it altogether, as there are, and must ever be cases requiring the entire extraction of the teeth, when disease has proceeded beyond a certain point, as for example, beyond the common cavity, which I have cursorily described; or when the jaw itself is diseased.

The instruments employed by me in this operation are forceps accurately fitted, like those for extraction, to the necks of the teeth, but having fine well-tempered cutting edges; these edges must be carefully applied on the necks of the teeth as close to the gums as possible, taking care to keep the edges parallel to the edges of the gums, which are to be depressed a little with the inferior surface of the blades of the forceps, so as to bring the cutting edges fairly beneath the enamel, which in the adult is the criterion of being below the common cavity of the tooth. Then, with a gradual application of pressure on the handles of the forceps, the tooth is in an instant snapped off at the neck, and the common cavity, the seat of pain, is thus removed, leaving the patient a painless bony surface for mastication, a firm prop for the support of the adjoining teeth, and a basis for an artificial tooth if it should be required. I may mention here a fact never before noticed, namely, that the openings by which the minute canals terminate in the common cavity, become, soon after the excision of the crown, *plugged up with bony matter*, which thus affords a permanent protection to the interior of the stump, and presents a continuous and firm surface for after-life. In addition to these advantages, the operation is performed even on the largest teeth in a moment, and, consequently, at a great saving of suffering, which should be the grand object of all sound surgery.—*Trans. Soc. Arts.* xliv. 70.

11. *Poisoned Wounds.*—At the Académie Royale de Médecine, M. Bouilland lately read a paper on some experiments relative to the effect of compression on poisoned wounds. Nine experiments were made. In the first five M. Bouilland introduced two or three grains of strychnia into the cellular tissue of the thigh of a hare, so that he could at will use compression on the limb, either by a ligature above the wound, or with an unexhausted cupping-glass, or with the hand alone placed on the wound; and he found that he could, by applying the compression or not, either prevent or produce the effects of the poison. He found, by many trials, the good effects of compression in causing the symptoms to cease or re-appear alternately according as he applied or removed the instruments.

In a sixth experiment six leeches were applied around the little wound, into which the strychnia was introduced, and it was remarked that, though none of them would bite, yet they all died.

In the three last experiments M. Bouilland used half a teaspoon-

ful of hydrocyanic acid, instead of strychnia, and found the same results. He concludes, i. That these effects furnish new arguments in favour of the idea, that many poisons are absorbed. ii. That their deleterious effect would be prevented by hindering their absorption, or arresting the circulation in the part to which they are applied, and that it is in this way the cupping-glasses used by Dr. Barry act. iii. That in cases of poisoned wounds, a ligature above the wound would have the same effect as the cupping-glasses.—*Med. Jour.* lvii. 84.

12. *Bites of Venomous Reptiles.*—M. le Dr. Record, sen., discovered, during a long residence in America, what he considers a sure mode of preventing mischief from such bites. "It is sufficient," he says, "to pour a few drops of tincture of cantharides on the wound, to cause a redness and vesication; not only is the poison rendered harmless, but the stings of the reptiles are removed with the epidermis that the blister raises."—*Revue Med.; Med. Jour.* lvii. 88.

13. *Poison of Rattlesnakes.*—M. E. Rousseau has ascertained, by experiments made with the fangs of a dead rattlesnake, that the venom of these animals preserves its injurious qualities for a long time, even in our climates, and at a very advanced part of the year. A pigeon wounded by them died in a very short period.—*Bull. Phil.* 1826, p. 141.

14. *On Two new Kinds of Urinary Gravel.*—These new varieties of calculi were observed by M. Majendie. The first occurred with a man of rank, a lover of good eating, who, being in circumstances in which he gave way to his inclinations, thought it right to eat each morning a large plateful of sorrel. After following this plan for more than a year, severe pains were felt in the loins and ureters, and shortly after a calculus was voided, six or seven lines in length, and two in width. It was hard, had an orange colour, and being analysed, was found to consist of oxalate of lime nearly pure. The oxalic acid introduced into the system by the sorrel was evidently the cause of this calculus, and an effectual remedy was found in change of diet.

The second kind of gravel was of much more uncertain origin. In this disease (as yet undescribed) the saline deposit of the urine assumed two forms, being sometimes a fine white powder, mixed with a large quantity of small hairs, varying in length from a line to an inch or more; and sometimes, on the contrary, forming white pieces, of unequal and irregular form, having no great degree of consistency, and crushing between the fingers. The fragments did not, however, separate entirely, but adhered together by means of a multitude of small hairs like those described, which, being mixed with, made a part of the mass.

Maceration separates these hairs from both varieties of the hairy gravel, as they have been named. They are then found to differ but little from ordinary hair, except in being finer, and of a grey

cinder colour. They are so numerous, that the smallest fragments exhibit their extremities, and, in certain instances, the surface of the stone is visibly covered with them. The accompanying matter being analysed, was found to be phosphate of lime, united to a small quantity of phosphate of magnesia, and uric acid.

Each of these varieties has been observed by M. Majendie but once; the patient from whom the first kind came rendered an enormous quantity daily. The phosphate of lime common to both varieties is, according to M. Majendie, a result of the excessive use of animal food; as to the origin of the hairs he does not even form a conjecture. The formation of these calculi was readily prevented by prescribing an almost exclusive regimen of vegetable food and alkalis.—*Bull. Univ.* c. iv. 297.

15. *Conquest of Peru and Mexico by the Mongols, in the Thirteenth Century.*—No historical discovery that has ever occurred, is more important than that which is just now published regarding America. In this volume we are told, that the Incas of Peru and the Kings of Mexico were derived from the generals of the Grand Khan Kublai, who sent a mighty expedition to subdue Japan; but a furious storm having dispersed the ships, many of them reached America, where the troops and the elephants landed; and these statements accord entirely with the histories and traditions of the Peruvians and Mexicans. The numerous identifications given in this work leave no room to doubt the truth of these assertions; and this curious fact accounts for many circumstances in the history of those two empires, which have hitherto puzzled and perplexed all those who have written on that subject.

The lovers of zoology will be peculiarly interested in thus finding that Mastodontes were living at so recent a period, their remains being found, mixed with those of the elephant, on the spots where the battles were fought, by troops who are decidedly known by the history not to have arrived in those kingdoms till the thirteenth century.

By what appears in this volume, in addition to a former work by the same author, a new light may be thrown on the subject of the quadrupeds employed in the wars and amphitheatrical sports of the Mongols and the Romans, all of which, it is rational to suppose, are still in existence. The inference of their being extinct has arisen from inquiries on that subject having been too much confined to geological, instead of historical examination, to the latter of which these particular quadrupeds more especially belong. The curiosity thus excited will open a wide field for a considerable addition to our present stock of knowledge on the important and highly interesting pursuit of zoological studies. It appears likewise that the Romans had the means to procure tapirs from Sumatra, and the long-sought unicorn is described from two living animals sent by an African monarch to the Sultan of Mecca, in the year 1503.

METEOROLOGICAL DIARY for the Months of December, 1826, and January and February, 1827, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For DECEMBER, 1826.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Friday	1	32	40	29.36	29.13	W	SW
Saturday...	2	33	44	29.00	28.90	S	S
Sunday	3	33	40	29.26	29.26	WbS	SW
Monday....	4	31	37	29.26	29.40	NW	NW
Tuesday ...	5	31	36	29.58	29.53	NW	NW
Wednesday.	6	32	45	29.48	29.53	SE	SW
Thursday ..	7	43	53	29.43	29.30	SW	SW
Friday.....	8	42	48	29.10	29.20	SW	SW
Saturday...	9	39	47	29.68	29.70	W	SW
Sunday	10	35	52	29.67	29.67	S	SbE
Monday....	11	47	51	29.62	29.67	SbE	SbE
Tuesday ...	12	40	49	29.67	29.48	SbE	SE
Wednesday.	13	42	47	29.36	29.40	SbE	SW
Thursday ..	14	38	47	29.40	29.40	SbW	SbW
Friday.....	15	42	45	29.43	29.48	SE	NE
Saturday...	16	42	46.5	29.48	29.54	NE	NE
Sunday	17	39	45	29.71	29.80	E	NE
Monday....	18	38	40	29.80	29.87	E	E
Tuesday ...	19	38	43	29.87	29.91	E	W
Wednesday.	20	37	42	29.80	29.49	S	SE
Thursday ..	21	35	40	29.51	29.90	NW	N
Friday	22	28	41	30.03	30.09	WbN	W
Saturday...	23	41	49	30.09	30.10	NW	NW
Sunday	24	44	46	30.19	30.19	NNW	NNW
Monday....	25	43	46.5	30.22	30.22	NW	NW
Tuesday ...	26	41	44	30.28	30.32	NW	ENE
Wednesday.	27	31	41	30.39	30.39	NE	NNW
Thursday ..	28	32	40	30.41	30.40	W	W
Friday	29	31.5	44	30.29	30.16	W	W
Saturday...	30	44	48	30.08	30.08	NW	NW
Sunday....	31	45	49	30.08	30.03	WbS	WNW

For JANUARY, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Monday....	1	41	48	29.80	29.50	WbS	WSW
Tuesday ...	2	25	38	29.40	29.47	WbN	W
Wednesday.	3	16	24	29.40	29.40	WNW	WNW
Thursday ..	4	9	27	29.50	29.60	WNW	N
Friday	5	24	30	29.98	30.17	NW	WNW
Saturday...	6	20	35	30.17	30.10	W	SW
Sunday	7	36	46	29.98	29.98	WSW	W
Monday....	8	41	52	29.86	29.76	W	W
Tuesday	9	41	44	29.65	29.61	W	W
Wednesday.	10	38	48	29.77	29.92	W	W
Thursday ..	11	34	47	29.18	29.16	W	W
Friday.....	12	34	38	29.23	29.68	NW	NNW
Saturday...	13	29	45	29.80	29.61	W	WSW
Sunday	14	45	52	29.21	29.44	SW	WNW
Monday....	15	30	37	30.00	30.10	W	W
Tuesday ...	16	37	47	29.92	29.84	WbS	WSW
Wednesday.	17	35	47	30.12	30.12	NE	NbW
Thursday ..	18	34	36	30.12	30.14	NbW	E
Friday	19	27.5	36	30.16	30.16	E	E
Saturday...	20	21	32.5	30.16	30.01	E	NW
Sunday	21	26.5	28	29.80	29.67	NE	ENE
Monday	22	24.5	33	29.43	29.60	E	ENE
Tuesday	23	24	30	29.57	29.50	EbN	NW
Wednesday.	24	28	32.5	29.41	29.50	WNW	ENE
Thursday ..	25	24	30	29.50	29.54	E	EbS
Friday	26	5	34	29.54	29.60	W	NW
Saturday...	27	28	32	30.00	30.10	NE	N
Sunday	28	18	38	29.98	29.80	SW	SW
Monday	29	39	44	29.73	29.70	W	WbS
Tuesday	30	36	42	29.64	29.60	SW	S
Wednesday.	31	36	46	29.58	29.55	SSW	SSW

For FEBRUARY, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Thursday ..	1	41	45	29.53	29.66	SW	NE
Friday	2	32	38	29.74	29.88	NE	N
Saturday...	3	29	33	30.26	30.40	NE	NE
Sunday	4	26	36	30.43	30.43	NE	NE
Monday....	5	19	33	30.40	30.29	NE	E
Tuesday ...	6	20	39	30.19	30.19	NW	NE
Wednesday.	7	29	37	30.21	30.28	ENE	NE
Thursday ..	8	26	35	30.29	30.37	NE	NE
Friday	9	25	34	30.32	30.29	NE	NE
Saturday...	10	27	37	30.12	30.00	NE	NE
Sunday	11	26.5	35	29.78	29.70	NE	NE
Monday	12	32	39	29.74	29.77	N	N
Tuesday ...	13	30	37	29.90	30.00	NW	NW
Wednesday.	14	24	36	29.93	29.82	W	W
Thursday ..	15	28	37	29.70	29.85	NW	N
Friday	16	15	30	29.97	29.97	NE	NE
Saturday...	17	12	30	29.80	29.74	W	NW
Sunday	18	9	30	29.87	29.89	SE	E
Monday	19	18	28	29.87	29.77	E	NE
Tuesday ...	20	16	32	29.64	29.60	NE	NE
Wednesday.	21	28.5	36	29.57	29.57	NE	NE
Thursday ..	22	27	37	29.67	29.83	NW	NW
Friday	23	19	34	29.87	29.87	W	W
Saturday...	24	28	38	29.80	29.80	W	NbE
Sunday	25	15	38	29.96	29.90	N	SW
Monday	26	35	49	29.65	29.50	SW	WSW
Tuesday ...	27	42	52	29.27	29.40	WSW	W
Wednesday.	28	29	46	29.58	29.27	NW	SW

TO OUR READERS AND CORRESPONDENTS.

THE pages of this Journal are impartially open to all communications upon the subjects of Science, Scientific Literature, and the Arts; and it is requested that they may be forwarded to the Editor, at least one month previous to the period of the publication of each Number.

We shall be happy to receive papers from Provincial Scientific and Literary Societies, and to publish them either on the part of the Society, or of their respective authors.

Papers which are too long for insertion, as is the case with many which we receive, or which are deemed unfit for this publication, will be immediately returned to the source whence we receive them, with our reasons for their return.

We are indebted to Mr. Merrett, of Liverpool, for the article on "the Architecture of London," and hope again to hear from him.

The communication upon the subject of Gymnastic Exercises we have been obliged to abridge more than we could have wished. It is chiefly extracted from the works of Mr. Shaw, surgeon to the Middlesex Hospital.

The short notice from E. S., upon the subject of Captain Head's "Rough Notes," was unfortunately mislaid, and not found again till too late for publication.

We cannot exactly see the advantage of the plan proposed by our correspondent at Droitwich; and, therefore, decline publishing his letter till we are more satisfactorily informed upon the subject.

Mr. Mac Mullen's additional observations upon the existence of chlorine in black oxide of manganese, arrived too late for insertion in the present Number.

TO READERS AND CORRESPONDENTS.

Reviews of the Philosophical Transactions, of Dr. Turner's Elements of Chemistry, of Mr. Faraday's Essay on Chemical Manipulation, and of several other scientific works, have been postponed for want of room. We shall certainly notice them in our ensuing Number.

The letter of F. R. S., and the paper signed "*Inquisitor*," are in reserve, as we conclude some *material change must soon take place.*

An account of the several Courses of Lectures delivered during the season in the Royal Institution, and of the proceedings of the general and other meetings of the members, was intended for this Number, but the pressure of other matter, and the length of our report of the weekly evening meetings, has obliged us to delay its insertion.

We are requested by Mr. Faraday to publish the following errors which occur in his work, entitled *Chemical Manipulation*, with their corrections. They are errors of the press, which are not self-evident, and might lead pupils into mistakes.

Page	83,	line	22,	*for*	39.37039,	*read*	39.37079
„	„	„	23	„	3.93704	„	3.93708
„	„	„	26	„	39.12929	„	39.13929
„	140	„	9	„	high	„	low

On the Supply of Water to the Western Part of the Metropolis by the Grand Junction Water-Works Company.

We are always ready and anxious to assist in redressing any public grievance which comes within the reach of our means, and the scope of our publication, but we must be left to do so in our own way, and upon perfectly independent principles: "nullius jurare in verba," is a sentiment which we have always adhered to, and which, upon the present occasion especially, must not be lost sight of. The little pamphlet * which has given rise to this article, is naturally calculated to excite much apprehension and alarm amongst the inhabitants of no inconsiderable portion of the West-end of London. It will be our business to determine whether its vehement asseverations are founded in truth—whether they are exaggerated—whether they are "wilful misrepresentations." It has been replied to by a short manifesto, circulated by the Grand Junction Company, and containing some statements which it will also be our business to sift and examine; and lastly, we shall lay before our readers the result of our own experience, observations, and experiments, touching the several matters alluded to.

The author of The Dolphin begins, as Mrs. Glasse advises, by first catching his fish; and gives some interesting and novel anecdotes of its habits and propensities:—"It is a certain fish of the whale tribe: the ancients consecrated it to the gods. Now-a-days, its boundings and springings are looked upon as sure prognostics of an approaching storm.—It swallows greedily whatever scraps are thrown to it; and is caught by a hook baited with garbage. Its sides are dingy, and it spouts water from a hole in the centre of its head." After this, and more of the same, the author proceeds as follows:—we shall condense and abridge the rest of the work, but we quote these paragraphs as a sample of its style and tenor:—

Of a widely different genus is "The Dolphin" which gives

* The Dolphin; or, Grand Junction Nuisance: proving that seven thousand Families, in Westminster and its Suburbs, are supplied with Water, in a State offensive to the Sight, disgusting to the Imagination, and destructive to Health.—"There is such a thing as common sense!" Abernethy.

birth to this little manual. Its virtues have been recorded neither by historian, philosopher, nor poet. It has never distinguished itself by any extraordinary attachment to the human race. Instead of bounding and springing, its position on the surface of the water is stationary. Instead of being fleshy, its head is ligneous. Like its namesake, it ingulphs immense portions of filth and garbage; but, so far is it from being esteemed a delicacy, that not only the Duke of Norfolk, but more than seven thousand families, at the court end of the town, have it constantly served into their houses.

Allegory apart—"DOLPHIN," in the Joint-Stock vocabulary, is the word used by certain conscientious gentlemen who exclusively supply the metropolis with that pabulum of life—WATER—to designate the source or head, in the river, from whence they draw the supply of the article in which they deal. To this Dolphin—to this wooden-headed, dingy-coloured, ill-shapen, insidious engine of destruction, fraught with more mischief to the inhabitants of Westminster and its suburbs than the wooden horse of the Greeks to the unfortunate citizens of Troy—to this box, more crammed with the seeds of all kinds of diseases—"*macies et nova cohors febrium*"—than that of Pandora, I entreat the earnest attention of every man who considers the Public Health an object of paramount regard.

That, at a moment like the present, when both the Legislature and the Government are endeavouring to unfetter the commerce of the country, by removing oppressive prohibitions and inconvenient restrictions, and thereby giving greater facility and encouragement to the skill, the capital, and the industry of the people of England—that, at such a moment, there should exist, in the very seat of that Government, a Monopoly of an element of nature, cannot but be matter of just astonishment.

There then follow some observations upon the penal laws applicable to monopolists, and of the mischief which has resulted from the well-known arrangement of the Water Companies; who, to prevent entire ruin, divided the supply of the town into districts, each limiting its service within a line agreed upon, and exchanging, with the others, the pipes beyond its boundary. "Thus," says the Dolphin, "were the customers counted out, and handed over by these jobbers in one of God's choicest blessings, from one set of monopolists to another, like so many negroes on a West-Indian estate, or so many head of cattle at a fair."

But we are principally anxious to call our readers' notice to the *facts*, or *supposed facts*, developed by the author of this pamphlet, and we shall adhere to his own arrangement of them; remarking, by the way, that the most serious and grave accu-

sations against the Company are frequently blended with such irrelevant and silly remarks, as to throw no inconsiderable suspicion upon their general veracity and correctness; as for instance in the following paragraph:—

With praiseworthy zeal, and laudable exactitude, they follow the instructions of Mrs. Rundell *, and other eminent professors, for the making of puddings, pies, broths, soups, jellies and caudle; and the boiling of meats, poultry, fish, and vegetables. They calculate, to a grain, the proportion that one ingredient ought to bear to another ingredient: they know, to the fourth part of a minute, how long each article should remain in the saucepan: but—*mirabile dictu!*—to the quality of the water of which those puddings are made, and in which those meats are boiled, the fair house-keepers of Westminster are less attentive—as I shall presently show—than the inhabitants of rude and uncultivated countries. In the integrity of their hearts, it never enters into their imagination, that any set of men should be so mercenary, so desperately wicked, as to sell to their customers a gift of Heaven, in a polluted and unwholesome state. They will now see, that their confidence has been abused, and that their indifference is culpable. —pp. 9, 10.

"Part I." of The Dolphin embraces three very important subjects of inquiry, under the general title of "the importance of pure and wholesome water for all domestic purposes." It first calls our attention to the "importance of this inquiry to the inhabitants of a thickly-populated metropolis:" and here we are told, upon the authorities of Mr. Abernethy, Dr. Paris, and other writers on health and diet, that most people are better for an occasional dose of country air; that exercise is essential to health; that pure water is an agreeable and wholesome beverage; that people continually confined within doors by sedentary business, are usually pale and sallow and dyspeptic; and that narrow alleys and dirty cellars are very comfortless and insalubrious.

As we are not indebted to the Grand Junction Company for the supply of foul air as well as of foul water, we cannot see the fairness of mixing up these two inquiries; however, the advantages of pure water are elaborately set forth a few pages

* Some idea of the demand for this lady's writings may be formed from the fact, that, first and last, *one hundred and twenty thousand* copies of her "New System of Domestic Cookery," have been printed; and of its companion, "The Family Receipt-Book," about fifty thousand.

further on, not upon the threadbare authority of the peptic empirics of the present day, but upon that of Hippocrates and Sir George Staunton, and of the "amiable Izaak Walton," and the unamiable Count Rumford. These conflicting evidences agree in the main point of showing that pure and clear water is much more agreeable than that which is loaded with filth and soluble impurities: and, as most of our readers will probably grant this point without further proof, we shall not waste their time in extended remarks; observing, however, by the way, that it was probably after reading the convincing facts detailed in these sections, that Sir H. Halford was induced to move, at one of the anti-junctionist meetings, "That a constant supply of pure and wholesome water is essential to the health and comfort of the inhabitants of this great and thickly-peopled metropolis!"

The second part of this pamphlet contains a common-place account of the different water-companies; and the third is particularly devoted to the Dolphin, or Grand Junction Nuisance: to this we must at present confine our attention, as it is here that the heavy accusations to which we have above alluded are embodied.

It is well known, that the different water-companies being upon the brink of ruin, entered into a mutual agreement to partition out the town amongst them: this bold, and we must call it, unconstitutional step was carried into effect towards the close of 1817, by the simultaneous retirement from each allotted district, of all the companies previously employed, except the individual one thenceforward to be left in the exclusive possession of the field.

The companies gave no previous notice whatever of their intentions: and, in reply to the remonstrances of their customers, they were informed, that, for the future, they could only be supplied by the one-continuing Company; and they were also given to understand, that an increased rate would be shortly exacted. The indecency of the proceeding produced a temporary burst of indignation; but, so deadening are the effects of monopoly, that the imposition was tamely submitted to, and the nefarious scheme brought to completion.

That a combination, bottomed in such a total disregard for the public welfare, should exist for any long period, without producing the train of evils ever attendant upon monopoly, was not to be

expected; but that, in the space of less than nine years, it should have given birth to the frightful enormity which I am about to expose, could not have entered into the imagination.

The Grand Junction Water-Works Company, as I have already shown, was not set on foot by any portion of the inhabitants of the metropolis, who felt themselves aggrieved by the conduct of the existing companies, but originated in a set of "Manchester gentlemen," holding a board in some alley in the City, and looking for their profits solely to an increase in the market-price of the shares, and by realizing the premiums which they could obtain upon a transfer of those shares.

That seven thousand families, in such a city as Westminster, should be bound hand and foot, and placed at the mercy of jobbers of this stamp, for a supply of an article, without the daily use of which they can have no life, is a state of things too frightful to contemplate. That it will be endured, when those families shall see how their comforts and their lives have been sported with by those jobbers, is impossible.—p. 40-42.

This is very strong language,—in our opinion much too strong: we wonder the author risked it; however, it is followed up by what may to some appear its justification, namely, a contrast between the *promises* and *performances* of the said Grand Junction Company. These we shall condense as far as is consistent with perspicuity.

They promise, by advertisement under the date of November 15, 1810, to supply abundance of *pure* and *excellent* water, even in the upper stories of houses; and to do this at a comparatively small expense: and in a second advertisement, they promise to give a supply so copious and regular, that the water is *always on*, *pure* in the pipes, and constantly *fresh*. They moreover undertake to make *no additional charge for high service*, and to avoid all delay in cases of fire, by keeping the pipes constantly full, and the supply always on. The water, they say, being perfectly *clear*, would not, in case of fire, *tarnish* the furniture. (This savours of quackery.) They then tell us that "the *annexed analyses* show the water to be peculiarly adapted to all domestic purposes:" but to us these analyses are sadly disappointing; in fact, they are merely a few lines signed by Mr. Aikin and Mr. Accum, attesting its general purity and fitness for domestic use; but what it contains, or what are its *impurities*, we are not told: this deficiency we shall therefore endeavour in due time to supply.

These, and others which we have not thought it necessary to transcribe, were the tempting *promises* held out by the Grand Junction Company. Come we now to their *performances* as enumerated by the author of The Dolphin.

No sooner was the confederacy completely established, than the Grand Junction Company began to give proof of the mischief ever attendant upon the possession of exclusive powers. Instead of the promised "daily supply" of water, it was sent into the houses of their customers only three days out of the seven. Instead of "increased security against the ravages of fire," it is a singular fact, that after almost every fire that has happened in the division, since the combination of the companies, complaints have been made in the public journals, of the much greater delay which has taken place, than was formerly known. Instead of selling it at a "comparatively small charge," they exacted, in August 1819, an increased rate, equivalent, in no case, to less than 50 per cent., and extending, in most instances, to 90 and 100 per cent. Instead of making "no additional charge for high service," an advance, in some instances, amounting to 100, 150, and even 200 per cent., was demanded.—pp. 49, 50.

We are afraid that most of this statement is not true, but we shall for the present reserve our own opinion, and hasten to the pith of the pamphlet, which is contained between the fifty-eighth and eighty-seventh pages.

It is here maintained that the Company instead of supplying the pure streams of the Colne and Brent, and valley of Ruislip, draw their water from the Thames, at the foot of Chelsea Hospital, and within a few yards of a large common sewer, thus sending to the inhabitants of London "a fluid saturated with the impurities of fifty thousand houses; a dilute solution of animal and vegetable substances in a state of putrefaction, alike offensive to the sight, disgusting to the imagination, and destructive to health." Now, to substantiate this thundering accusation, we did hope that the author of The Dolphin would have had a careful analysis made of the said water, and that its results would have justified the violence of his assertions. But no such thing; his analysis is still less satisfactory than that which we have above complained of, as published by the Company. It was performed by Mr. Joyce, the successor of Mr. Accum, and the following is its amount:—"The water *sent for assay* was found to be loaded with decomposed vegetable matter, and in such quantity as to be unfit for use without

tedious purifications." The right way of proceeding in these cases would have been, not to send a bottle of the water in question to the first person that occurred, but to have requested one or more respectable chemists to have examined the water as coming directly from the mains of the Company, and to collect and experiment upon it in their own way. We think, therefore, that no weight whatever can be attached to the above, or any analogous analysis; nor are the *opinions* of the medical men which follow of much more importance, for they are but opinions after all, and those upon one side of the question only, for we could easily obtain a similar number of equally respectable certificates in favour of the water, as are here so formidably arrayed against it: however, it is but fair to let some of these gentlemen speak for themselves.

Mr. Abernethy here stands forth in the foremost rank, to whom our author addressed a letter, *with a sample of the water*, informing him, that instead of pure water the Grand Junction Company supplied a fluid loaded with all sorts of filth and impurities, drawn from within a few yards of the great common sewer: he then proceeds to inquire whether such water is not more fit to engender disease than to make soups and puddings. Now for the answer:

Scarcely had I put the above letter into Mr. Abernethy's hand, when I unfortunately held up the specimen bottle, and asked him, "whether he thought such water could be wholesome." Never shall I forget the countenance of this eminent man at that moment! The very sight of the turbid fluid seemed to occasion a turmoil in his stomach. He began pacing the room backward and forward, and the only words I could extract from him were, "How can you ask me such a question? How can you ask me such a question? There is such a thing as Common Sense! There is such a thing as Common Sense!"

Mr. Pope tells us, that

> "Great wits sometimes may gloriously offend,
> And rise to faults true critics dare not mend—"

and so may great professional men. There was a sort of honest *brusquerie* in Mr. Abernethy's conduct, that pleased me beyond measure; and I left his house, satisfied that I had not only established my case, but supplied my intended publication with an excellent motto.

In a communication which I have since been favoured with from Mr. Abernethy, he has had the goodness to confirm my interpre-

tation of his exclamation, and to express his sincere wishes for the success of, what he is kind enough to call, my "public-spirited exertions."

We have transcribed this "Scena" between Messrs. Abernethy and Wright, inasmuch as the latter gentleman has placed it first upon his medical evidence, and has taken from it the motto of his pamphlet. The impression that it leaves upon us is, that if there be such a thing as "common sense," there is also such a thing as nonsense, and that Mr. Wright is wrong in publishing such rhodomontade.

Now for the rest of the medical opinions. Dr. William Lambe says, "the water must be unwholesome;" but Dr. Wilson Philip more circumspectly observes, that "good water is essential to health." Mr. Thomas and Dr. Hooper represent the use of the Grand Junction water as prejudicial to health, and the latter, especially, complains of the live insects in which it abounds; and Mr. Brodie conceives it to be unwholesome, and altogether unfit for culinary purposes.

Dr. Paris says that the water in question is extremely impure and unwholesome, and that he cannot find terms expressive of the awful effects it may be likely to produce upon the health, and even lives of the inhabitants of the Metropolis.

Mr. Keate is yet more decided: he says the water is so filthy and impure, as to be unfit for the breakfast table, or for culinary purposes; and that it adds so much to the other impure and unwholesome constituents of bread, as to render every meal injurious to the health of thousands. He adds, "by the aid of filtering machines, and a steam kitchen, I endeavour to avert from my family the mischiefs and dangers which I should otherwise apprehend from the use of the sad compound which is laid into my house."

Doctors Turner, Hume, Macmichael, Holland, Bree, and Johnson, close this formidable catalogue with evidence couched in nearly similar terms to that above given; but Sir H. Halford is somewhat more circumspect: the following is his reply to Mr. Wright:—

"*Curzon Street, March* 1, 1827.

"Sir,

"I have been disgusted, for some time past, by the filthy fluid which has been served to my house by the Grand Junction Water

Company; and, although I am not prepared to prove that its influence on the health of the inhabitants of the west-end of the town *has* been deleterious, I conceive it likely to *become* so, if it continue to be supplied in the same foul and muddy condition, in which it comes into our houses at present.

"I must add, Sir, that I think the Public is under great obligations to you for bringing this subject of the supply of water to the Metropolis, to the notice of Parliament.

"I am, Sir, your's truly,
(Signed) "HENRY HALFORD."

Now, throughout this evidence, two circumstances have forcibly struck us, and greatly tend, in our opinion, to invalidate its evident object,—the first is, that no analysis of the water was undertaken by, or at the request of, any of the above gentlemen; that no chemist was consulted, and that we are studiously kept in the dark as to its nature and composition;—the second, that not one actual instance of the poisonous and mischievous qualities of the said water is adduced, or even hinted at; nay, even the "Doctors militant" upon the above formidable list are, themselves, not the least healthy specimens of the inhabitants of the parishes of St. George and St. James, though they have now, for several years, had no other aqueous supplies than those of the horrible common sewer to which they refer.

The fourth and last part of The Dolphin, proposes to treat of the "abatement of the nuisance, and of the best mode of procuring a pure and wholesome supply of water for Westminster and its suburbs." To this we turned with much eagerness, in the hope of finding some plausible suggestions or practicable plans for the improvement of the supply of water to the Metropolis generally; but the author has here merely recapitulated his former statements, without suggesting any thing plausible in the shape of remedy for the grievances which he has detailed: the remedy, indeed, is not very obvious; but before we venture to throw out a suggestion or two respecting it, we are in duty bound to make a few observations upon the conduct of the directors of the Grand Junction Water-Works Company.

At a meeting held at Willis's Rooms, on Monday the 9th of April, "to take into consideration the means of supplying pure and wholesome water to the inhabitants of the western portion of the Metropolis," certain resolutions were passed, which we

have annexed in a note,* and upon which the directors have published a series of "remarks," some of which are much to the point.

In the first place, the said directors have been held out to their customers as a most obdurate and inflexible body of gentlemen, quite inaccessible to all remonstrance, and deaf to the reiterated complaints of their customers; but here we find, that immediately upon a notice being given in the House of Com-

* "RESOLVED:

"1st. That a constant supply of pure and wholesome water is essential to the health and comfort of the inhabitants of this great and thickly-peopled metropolis.

"2d. That the principle of the Acts of Parliament, under which the several companies supplying the metropolis with water were instituted, was to encourage competition; seeing that it is only from competition, that a perfect security can be had for a good, a cheap, and a plentiful supply.

"3d. That, nevertheless, by an arrangement entered into, about the year 1817, between the several companies supplying the metropolis with water, all competition was put an end to, and a monopoly of this necessary of life virtually established.

"4th. That the water taken up from the river Thames, at Chelsea, for the use of the inhabitants of the western portion of the metropolis, being charged with the contents of the great common sewers, the drainings from dung-hills and lay-stalls, the refuse of hospitals, slaughter-houses, colour, lead, and soap works, drug-mills and manufactories, and with all sorts of decomposed animal and vegetable substances, rendering the said water offensive, and destructive to health, ought no longer to be taken up by any of the Water Companies from so foul a source.

"5th. That the Grand Junction Water-Works Company, having engaged to supply their customers with water of the purest and most wholesome quality, to be drawn from the rivers Colne and Brent, and from an immense reservoir of nearly one hundred acres, fed by the streams of the Vale of Ruislip, have, nevertheless, since the month of September 1820, drawn their supply from the Thames, at the foot of Chelsea Hospital, and nearly adjoining to the mouth of the Great Ranelagh Common Sewer.

"6th. That the water supplied by the Grand Junction Company, to more than seven thousand families, has been pronounced, by professional men of the first eminence, to be a filthy fluid, loaded with decayed vegetable matter and other substances equally deleterious to health, and unfit for domestic purposes.

"7th. That the Grand Junction Company, having promised to supply their customers with water, at a comparatively small charge, have, nevertheless, exacted an increased rate, equivalent, in no case, to less than fifty per cent., and extending, in most instances, to ninety and a hundred per cent.; and that they obtained, in May last, the sanction of the legislature to a new table of rates, by which an addition of fifty per cent. may be levied on their customers.

mons, by Sir F. Burdett, of his intention to move for a committee of inquiry into the conduct of the Water Companies, the chairman of the Grand Junction Works, requested an interview with that gentleman, and stated to him the *readiness and anxiety* of the directors to afford the means of the fullest investigation to any committee or deputation of householders in their district of every point connected with the powers of supply, or rates charged by the Company. To this proposition, Sir Francis did not accede; and the directors say that they have neither the right, nor the wish to complain of his determination; but the householders, in our opinion, have both, for we are assured that many of the most respectable in the district would have been very glad of such a conference, in whatever way it might have terminated, and that the impression upon their minds, apparently warranted by the silence of the Company, was that the latter blinked the question, and would condescend to no compromise.

The Company's "Circular," after some remarks upon their charges, which we, at present, shall not meddle with, tells us, that *until they ceased to take their supply from the Grand Junction Canal, frequent complaints were made of the quality of the water, and the directors were constantly importuned to resort to the Thames.* Now, this statement is not very consistent

"8th. That a petition be presented to both houses of parliament, praying that a full inquiry into the constitution and practices of the several companies supplying the metropolis with this all-important necessary of life may be instituted; in order that the nuisance complained of may be speedily abated, and the supply of pure and wholesome water to the western portion of the metropolis be henceforward placed on a sure and lasting foundation.

"9th. That a subscription be entered into to defray the expenses which must be incurred in preparatory measures connected with the object of the present meeting, and that the following noblemen and gentlemen be a committee to receive and appropriate the same (Here follows a list of the names of the committee):—

"10th. That the thanks of this meeting be given to Mr. J. Wright, of Regent-street, for having directed the public attention to this important subject.

"11th. That Sir Francis Burdett, Bart., and John Cam Hobhouse, Esq., our representatives in parliament, be instructed to support the prayer of the said petition.

"12th. That the thanks of this meeting be given to Sir Francis Burdett, Bart., for his impartial conduct in the chair.

(*Signed*) "FRANCIS BURDETT."

with their former eulogium upon the pure streams of the Brent and Colne, and of the immense reservoir, fed by the streams of the Vale of Ruislip; all which formed the grand decoy-duck of their original manifesto, and induced their customers to hope for a supply of water purer than that of the purest part of the Thames.

In reply to the direct charge of the nastiness of their water, that is, to the sixth resolution of the public meeting (see page 258), we have the following statement—we must apologise for the length of the quotation, on account of its importance:—

"The directors will commence what they have to observe upon this head with admitting frankly, and at once, that, during a certain period of the year, the water of the Thames is not such as in their opinion ought to be delivered to the inhabitants of London, —denying at the same time most distinctly that the foulness of the water supplied by the Grand Junction Water Company, at such times, is attributable to any other cause than the turbid state of the river during the prevalence of floods, or has been greater in degree, or different in quality from the foulness observable in the water supplied by other companies during the same periods: upon this head the directors can well afford to show the most complete candour, when they add (what they will presently more fully allude to, and hereafter establish by evidence before the committee) that, to remedy this evil, they have advised with the most eminent engineers, and have devoted the patient and laborious attention of between two and three years, and an expenditure little short of 50,000l. Whilst admitting thus much, however, they cannot but animadvert (and they do so with regret) upon the loose mode in which the opinions of some individuals, alike eminent for professional skill and private respectability, who spoke at the meeting at Willis's Rooms on the quality of the water, seemed to have been formed. That by the person who has excited the public mind on this matter, miserably unfair means should have been taken to obtain from professional men, whose names have justly great weight with the public, opinions unfavourable to the quality of the water, was only what the directors might have expected, and is consistent with the whole tenor of his conduct as regards this Company; but that, in a public meeting, respectable individuals should have spoken in strong terms upon the bad quality of the water, without even knowing in what state it was at that moment flowing into their own houses, the directors have perceived with no inconsiderable surprise.

"One highly respectable individual, whose judgment, from his having recently paid attention to this subject, cannot but have had much influence, and who spoke at the meeting with great animation upon the foulness of the water, the directors have reason

to believe, must have formed his opinion, not from the supply of the Company, but from the contents of a cistern in want of cleansing.

" To neglect in cleansing out cisterns they have indeed, in numberless instances, been enabled to trace a complaint of the quality of the water, and they do in consequence protest most decidedly against any judgment upon the water supplied by the Company, unless it shall have been formed from specimens fairly taken, and in the presence of their own officers. Without going at present into greater length to point out the causes which may have misled the judgments of the respectable authors of the very extraordinary opinions which have been published upon this head, the directors pledge themselves to prove satisfactorily before the committee the utter incorrectness of those opinions.

" In fact, whilst it is true that, during a part of the winter, water drawn from the Thames is turbid, it is no less true that, except during floods, it is, even when fresh drawn from the stream, exceedingly good, and that the water served by the Company is at this moment, as it has been for many weeks, bright and pure.

" To be foul from floods during a portion of the year, is of course common to all rivers, though not in an equal degree: the waters of a large river, and which derives its supply from a vast extent of country, will be less likely to become completely turbid than the waters of a smaller stream, all the sources of which may be affected by the influence of one heavy rain; and the directors are well persuaded, that of all the rivers that could be made available for the supply of London, the Thames ranks decidedly the first; indeed it was reserved for this occasion to impeach the quality of the waters of this noble stream, which has hitherto been supposed to stand as unrivalled in this as in all other respects.

" To remedy this defect, and to enable the Company to supply bright water at all seasons, the directors have for now nearly three years been devoting an unremitting attention. In 1824 the opinions of three of the most eminent engineers were taken as to the best mode of effecting this object; two of them recommended additional reservoirs at Paddington, with a feeding main from the river, distinct from the mains that supply the Company's district from the reservoirs;—the third recommended reservoirs at Chelsea: in either case it was pronounced the Company would possess unrivalled means of affording to London a supply of water pure and abundant.

" The Company in the end adopted, not *one*, *but both* of these plans; and the additional works were commenced in the beginning of 1825. The Company now possess, at Paddington, three reservoirs of eight acres, and are about to make three additional reservoirs, at Chelsea, of four acres: when all are completed, they will afford such means of giving a supply of pure water, as have never yet been equalled, or, they might say, even approached in ancient or modern times.

"The water, from the time allowed for repose, will be bright at all seasons, whilst from the ample reserve at either end of the line of communication (Chelsea and Paddington) the supply of the town will run no risk of being disturbed by accident to any part of the works."

Lastly, the directors state, and our analysis of the water proves them correct, that all that has been said respecting the influence of the Ranelagh drain upon the supplies drawn through their Dolphin, is either utterly false or grossly exaggerated.

"1. The Dolphin is so placed with reference to the Ranelagh Drain, that it cannot possibly be affected by the drainage water; during the ebb tide not a drop can come near it (unless it were to flow against the stream), and during the flood, the rising water of the river stops the egress of the current of the drainage.

"2ndly. If the water delivered by the Company were affected by the Ranelagh drainage, it would be more affected in summer, when less water came down the sewer to dilute its impurities, than during the floods:—on the contrary, the water supplied by the Company in winter was turbid in common with the supply of the other companies, and now, in common also with their supply, it is bright.

"3rdly. If when the Dolphin were first placed in its present situation, it had been supposed that the supply of the Company could be affected by it, is it credible that men of sound mind would have permitted it to be so placed? or what temptation could the Company have had strong enough to induce them to disgust the town, which it was of such paramount importance to them to conciliate."

We shall now offer a few brief remarks upon the above discussion, promising to keep an eye over the parties concerned, and to resume the subject whenever occasion requires; premising that we are actuated by no kind of antipathy to Mr. Wright, nor affection to the company. We trust, in short, that we are, and shall remain, "candid and unprejudiced observers of passing events."

First, we must in justice allow that the supply of the Grand Junction Water Company is *frequently* unexceptionable that it is abundant, clear, and free from any smell or taste, and quite fit for all domestic and culinary applications; and we set out with this statement, verified by repeated trials, and analyses, to show that, if they choose, the Company have the power of supplying unobjectionable water. To what extent

this power may be limited, or by what circumstances it is influenced, we recommend them most seriously to investigate and consider.

But, secondly, it must be admitted, that the water is also frequently sent into the houses in a very objectionable state, extremely turbid, abounding in shrimps, worms, and other aquatic insects. Here is the grievance; people are disgusted at seeing live animals twisting about the bottoms of their pitchers; and the diffused mud gradually subsiding, fouls the cisterns, in which, if not repeatedly cleaned, it soon constitutes a most annoying deposite; it is necessarily stirred up and diffused whenever the water comes in, and often putrefies, rendering the whole contents of the cistern fetid and unwholesome. All this the Company allow; but they tell us, that the cisterns should be frequently cleansed: but this is often no easy operation; it is requisite at least once a month, and is constantly leading to the disturbance of water-closet machinery, and other expensive inconveniences. Besides, many cisterns are not easily accessible. We also must here beg leave to remark, that if the Company did their duty towards the public, they never would throw in their water, thus loaded with mechanical impurities; they would make such arrangements of reservoirs, as might enable them to supply such water only as had completely clarified itself by deposition, and we should then suffer so little inconvenience from sediments in cisterns, that an annual cleansing would probably be sufficient.

This grand desideratum we are happy to say the Company promise to effect, as will be seen in one of our preceding quotations from their "Circular;" and if they keep their promise, and we see no reason why they should not, we are convinced that the majority of their unprejudiced opponents will withdraw from the field.

But there are certain theoretical ultra-chemists who tell us that all such precautions go but little way to amend the mischief; and that though mechanically suspended impurities may be separated by subsidence and filtration, the matters which are actually dissolved in the water can never be got rid of by any such operation: they then go on to represent the horrible

things which *may* thus be held in solution, but are never at the pains of actually experimenting upon the subject, which might enable them to state what really *is* there found. The result of several experiments upon the Grand Junction water leads us to believe that it is not objectionable when its mechanical impurities have been separated; we have found none of that abundance of animal and vegetable matter which the said chemists had led us to anticipate. Carbonic acid, carbonate of lime, a little sulphate of lime, and some common salt, are the leading ingredients which we have detected; and these, in no alarming relative proportions to the whole mass, a pint of the water yielding upon an average a grain and a half of soluble matter, and always less than two grains. We therefore repeat, that whenever the Company have so far improved their arrangements as to enable them to supply no other water than that which has completely deposited its suspended matters, there will be no longer any grounds of complaint, provided they neither increase their rates, which are already enormous, nor diminish the quantity, nor alter the regularity of their service. We have been at the pains of examining water from other parts of the river between Chelsea and London Bridge, and the results are not unfavourable to the statements of the Grand Junction Company; it is neither chemically purer, nor mechanically cleaner. In the supplies of the West Middlesex Company we have not discerned any important shade of improvement; the water is neither softer nor cleaner. To conclude, we have no hesitation in saying, that the only source whence the metropolis can obtain river water in sufficient abundance, and of unobjectionable purity, is from the Thames, and the higher up the river the better. Some years ago a plan was suggested for carrying Thames water from the vicinity of Teddington to the top of Hampstead Hill, and thence supplying London and its environs; it was deemed visionary, and laid upon the shelf. The fact is, that the difference between the water there and at Chelsea, is not such as would justify the outlay of a million of money, and a proportionate increase of the rates.

On the Cultivation and Improvement of Edible Fruits.

THERE is nothing, perhaps, in the whole range of human exertion which equals, or that exhibits the value and utility of the horticultural art so conspicuously, as the improvements which have been made in the melioration of our edible fruits. Not one of the numerous kinds and varieties of fruit which our gardens and orchards produce, are what they were in their aboriginal state; and several there are, which appear to be absolutely new creations:—the offspring of accident or skill, rather than the spontaneous productions of nature.

To present the reader with a general view of this curious and interesting subject, is the object of this paper; and it is intended to be, not only a theoretical display of the effects of cultivation, but also a practical exposition of the means employed to obtain such results,—marking the steps which have progressively led to the completion of the present system of pomological improvement; and, in order to make the detail as concise as possible, it will be necessary to trace the efforts of the first cultivators in the management of their favourite fruits, which will lead to the consideration of all the expedients now practised for the purposes of improvement.

It may be naturally supposed that the native fruits of the woods were first transplanted from their indigenous stations to be, as it were, domesticated near the habitations of man:—here they received protection, and a certain degree of cultivation; were free from the encroachment of other trees, and weeds, and had other care bestowed upon them.—This treatment would increase their growth, enlarge their fruit, and, perhaps, improve its flavour:—thus repaying the first attention of the planter.

We may next suppose, that the planter, finding the fruit useful, or perhaps necessary to his existence, might imitate nature in his desire to increase his primitive orchard, (if no suckers rose from the roots,) by saving and sowing the seeds of his protegées. Among the produce, he might observe some individual seedlings of stronger growth, and broader leaves than the rest; to such he would, very probably, give a preference, not from previous experience that such would be superior sorts, but merely

from their more kindly appearance and habit. Haply, they might turn out improvements of the original; and, elated with his success, would repeat his experiment, but in succeeding trials, he might not be so fortunate; and would be driven by his wishes, to devise some other scheme to extend or propagate his already acquired favourite fruit. In this pursuit he might, perhaps, have noticed, that parts of plants severed from the parent, if placed by accident in the ground, and in a suitable situation, would strike root, grow, and become a tree, in all respects like that which produced it:—or he might observe, (which is a circumstance more frequently seen) that branches of trees, partly buried in the soil, would become trees after a time, independent entirely of their original. On these circumstances, our modern custom of propagation by cuttings and layering, have undoubtedly originated; and no doubt our primitive gardener, witnessing those instances of reproduction, would be insensibly led to the knowledge of some of the properties of plants, and soon would arrive at the cognizance, that some were individual essences, and that others were a congeries of essences, distinct in themselves and from each other, and divisible without destruction to each other.

When, however, with this slender stock of knowledge, and means of propagation, any of these processes became impracticable, or inconvenient, he would be urged on to other expedients for the extension and perpetuation of his improved fruits;—in the course of time and experience, he, or some of his juniors, in the same pursuit in succeeding generations perhaps, might hit upon the idea and practice of grafting. This discovery would be considered as a prodigy, as it would be productive of unbounded facilities in the propagation and distribution of improved fruits; and would lead to most beneficial results, in every department, and for every object of the pomologist's art.

The practice of grafting is of great antiquity; but the exact period of time or place when it was first performed, is unknown:—for though gardens have been described, both by sacred and profane writers of the earliest ages; and though they have been the theme of kings, and priests, and prophets, as well as poets;—as paradise—Hesperides—and Paphian bowers; yet of grafting, the earliest annals are silent. It appears, however,

that it (as well as the effects of it) was known eighteen hundred years back :—witness what St. Paul has written in the eleventh chapter of his Epistle to the Romans ; from which passage, it is to the English reader, evident that something like grafting was known in his days. I am, however, not aware of the strict meaning of the Greek verb used by the Apostle in this passage, and which is rendered "grafting," by our translators. But however this may be, is a point of no importance in the present detail; the art itself is one of the most useful operations in the management of trees, not only to perpetuate the most approved kinds of fruit for a long series of years, but is also productive of great advantages in the right adaptation of the stock and graft, and by which they can be made to counteract the exuberances of each other.

Another remarkable process of improving fruits has occurred to cultivators, since the discovery of the sexual system of plants by the celebrated Linnæus.—That there existed such a thing as sex in the vegetable kingdom, was known, or rather apprehended before his time, is sufficiently evident from the writings of naturalists; but it remained for his eminently scrutinizing mind to confirm and illustrate this curious fact, in the economy of nature. On this, he arranged his system and classification of plants; and though it be a more artificial (and too homely, perhaps) mode of distinction than that of some other botanists, and is, consequently, rather giving way to the more natural arrangement of Jussieu; yet the leading fact of Linnæus's system is so far proved, and taken advantage of by improvers, that many valuable varieties of new fruits have been obtained, by simply impregnating the female parts of one kind with the farina of another; thus producing a new progeny, which is neither one or other, though partaking of the qualities of both.

It is astonishing how far this means of improvement and fertilization has already been carried, and inconceivable how extensively it may be exercised in the further amelioration of fruits ;—the prospect is boundless, and future generations only, will witness the consequences of this easy and certain method of improvement! To the silent and unheeded operation of this circumstance, whatever may have been the agents, we are doubtless indebted for many varieties, which have been found

in situations far from all interference of art, and brought into existence by fortuitous causes alone.

The fertility of plants, as well as the quality of the fruit, can also be improved by checking the luxuriancy of the growth.—This is accomplished in various ways;—by frequent transplantation—by wounding or cutting off some of the principal roots—by circumcision, or ringing the stem, or branches—by double working, *i. e.*, grafting or budding twice or thrice on themselves—and lastly, by planting in light, shallow, dry soils. The effect of all these violences is to check the cresive energies of the plants, and, consequently, *and necessarily*, bringing the fructiferous or reproductive essences into action. Why this should be so, is a curious question, and can be better answered by the anatomist, than it has hitherto been by vegetable physiologists. An exactly similar case exists in animal economy; and if a reason can be given for the one, it will equally apply, by analogy, to the other. The robustly corpulent and obesious are not the subjects of facilitous fecundity.

There remains to be noticed the external means which have been practised for the maturation and preservation of fruit-trees; and these are, defending them from the attacks of insects; placing them in such soils and situations as are most congenial to their natural constitutions; and protecting them from the extremes of the weather, by walls and skreens of hedges, &c.; and for the more delicate fruits, providing glass cases,

> " Where blooms perpetual spring, and summer shines
> " In months not hers ———————— "

in which all the fruits of our own clime, as well as some of the most valuable of all others, are brought to the greatest perfection; and even rival, in excellence, those produced under the influence of their native skies.

We next proceed to show the effects and results of the above-noticed means of improvement; and beginning among the more humble growths, notice first—

The Strawberry.—This favourite, fragrant, and delicious fruit is found wild over this, and many other countries of the globe. In its wild state, it is found in woods, and on hills; hence the two grand divisions of sylvan and alpine strawberries.

Naturally, their growth is diminutive,—the fruit small,—and, except on very dry situations, very tasteless. When transplanted into the garden, where they have the advantage of a rich soil and suitable cultivation, they are improved in all respects:—many varieties have been imported from other countries, and a far greater number have been obtained in this, chiefly from seeds properly prepared by cross impregnation: by which means, the strawberry has been wonderfully improved; instance the hautboys, scarlet, chilli, but particularly the splendid varieties, called "Wilmot's superb," and "Keen's seedlings."

Of the Raspberry.—This is also found wild in the British isles, on its native site, (with its companions, the bramble, and dewberry)—its shoots and fruit are diminutive, though the flavour of the berry is rich. No plant requires the skilful hand of the pruner more than this; of all others, it is, perhaps, the most viviparous, throwing up, annually, a vast redundancy of shoots, which, if not displaced at the proper season, would impoverish not only the fruit of the present, but also the bearing wood of the next year. The flavour of the fruit is much esteemed, and no product of the garden is of more use to the confectioner. The Dutch fruiterers have been successful in obtaining two or three fine varieties from seeds; and as this field of improvement is open, no doubt further exertions will bring forth new and valuable sorts.

Of the Gooseberry.—This is another indigenous production of this country. In its wild state, we find it a low irregular bush, thickly set with spines, and bearing small, rough, insipid berries. When introduced into the garden, the richer soil, pruning, defending from insects, &c., produces a wonderful change; the growth becomes more robust and luxuriant, the leaves are larger, and the fruit is increased in size, improved in flavour, and the whole plant assumes a new character; no domesticated fruit sports into greater variety than this: the endless lists of new sorts is a proof of this, and many large and excellent sorts there are, particularly the old Warrington red.

Of the Currant.—The native place of this useful fruit is not exactly ascertained; nearly allied to the gooseberry, it receives the same treatment, shows the same changes, and may be further improved by the same means; a cross between the

white Dutch and red, might be a valuable mule. It is probable the black also may be induced to sport from that steady character it has hitherto maintained; there are but few domesticated plants but which (like animals) depart, in some way or other, from their native caste.

Of the CHERRY.—Cultivation has accomplished wonders in the improvement of this beautiful native fruit. Instead of a lofty forest-tree bearing small bitter fruit, it has been long introduced to our orchards, is changed in appearance and habit, and even in its manner of bearing; has sported into many varieties, as numerous as they are excellent—nor is such improvement at an end: several new varieties have lately started into existence, children of the experiments of the first orchadist in the world, whose practical abilities are only equalled by his superlatively scientific knowledge, and whose extensive usefulness in rural economy can only be compared with his amenity of character as an English gentleman; and who, it is hoped, may long live to complete his numerous improvements, and to lead and direct the current of pomology, which is now, more than ever, finding its way over the whole land.

Of the PLUM.—The lowest grade of this class of fruits is the almost useless sloe in the hedge; and none but those in some degree acquainted with the matter could, on beholding the acidous, puny sloe, and the ample, luscious magnum bonum plum, together, readily believe that they were kindred, or that the former was the primitive representative of the latter. The intermediate links of this connection are the bullace, muscle, damacene, &c., of all which there are many varieties. In nurserymen's lists, there are many improved sorts, not only excellent plums, but excellent fruit,—the green gage and imperatrice are admirable; and as it is more than probable that many of our dessert sorts have been the mere offspring of chance, what may we not expect, when the ingenuity and judgment of the practitioner is applied to accomplish such an end?

Of the PEAR.—This well-known garden-fruit was originally an inhabitant of European forests: there it grew to be a middle-sized tree, with small leaves, and hard, crude-tasted, petty fruit: since its introduction and naturalization in the orchard, it has well repaid the planter's care: the keeping quality of the fruit,

and the thirst-quenching virtues of its juice, have made it universally a favourite. The French gardeners have been long celebrated for their success and indefatigable perseverance in the cultivation of the pear; almost all our superior sorts are from that country. The monastic institutions all over Europe, but particularly in France, were the sources from whence flowed many excellent horticultural rules, as well as objects. The great variety of this fruit all over Britain, and the still greater diversity of provincial names, have rendered our lists most inexplicable; but it is hoped that such confusion and misnomers will not be allowed much longer to exist.

Of the Apple.—It is difficult to find adequate terms to set forth the value of the advantages which have accrued to mankind from the cultivation of this deservedly high-prized fruit. In our woods and hedges, the crab and wilding are everywhere found; crooked, hostile, rigid in figure and quality;—with numerous, small, austere fruit, which even the hog will hardly eat; but, transplanted into the garden, and subjected to the corrections of the husbandman, they have, in the course of time, been divested of their savage character,—forming a goodly tree, and yielding crops of goodly fruit, fit for many purposes of life. Many of the newest sorts have been spontaneously produced, and a great number by the art of fertilising the flowers of one with the pollen of another. One circumstance in the history of the apple must not pass unnoticed here; viz., the deterioration of the old sorts, which regaled and were the boast of our forefathers a century ago. It is the opinion of an eminent orchardist that as the apple is an artificial production, and, as such, has its stages of youth, maturity, and old age, it cannot, in its period of decrepitude, be by any means renovated to its pristine state, either by pruning or cutting down, changing its place, or by transferring its parts to young and vigorous stocks; and that, in whatever station it may be placed, it carries with it the decay and diseases of its parent. This is the most rational account which has been given of this indisputable fact; and though its accuracy has been called in question by some naturalists, the general failure in our old orchards, and the difficulties in forming new ones with the old favourite sorts, is a decisive proof that such deteriora-

tion exists. It is therefore the chief object of the modern pomologist, to obtain from seeds of the best *wildings* new varieties wherewith to form new and profitable orchards; and which may be expected to continue in health and fertility, as the old sorts have done, for the next century. The preservation of this useful fruit is now brought to great perfection, by keeping them in jars secured from the action of the air: but there is one method of preserving them for culinary purposes, which, as it is not practised in this country, may be noticed here. Any good baking sort, which is liable to rot, if peeled and cut into slices about the thickness of one-sixth of an inch, and dried in the sun, or in a slow oven, till sufficiently desiccated, may be afterwards kept in boxes in a dry place, for a considerable time; and only require to be soaked in water for an hour or two, before using.

The foregoing are the fruits found wild in our climate; the difference in their aboriginal and cultivated state has been pointed out; we shall now give short descriptions of foreign fruits, which have been partly naturalised, the management of which forms so considerable a share of the gardener's art and attention.

Of the Apricot.—It is supposed that this fruit is a native of Africa: from thence it appears to have come through Persia and Greece to us, with the name "a præcox," significant of its earliness. It is one of our wall-fruits; though, in favourable situations, it will ripen its produce, either planted as a standard, or trained as an espalier. There are several varieties which have been obtained by means similar to those already mentioned; and there is room for further exertion in endeavouring to improve the size of the fruit, or any other desirable quality.

Of the Peach.—This delicate and excellent fruit is a striking instance of what judicious cultivation may produce. The common almond has always been considered the original stock of this monument of skill and assiduity. The estimation in which it is held, and the care and expense incurred in its cultivation both in forcing-houses and in the open air, is proof of its superiority: and no fruit repays the labour of the attendant, or expense of the owner, more bountifully than this.

Seedlings of this fruit are, if we can credit what is written and said of it, less inclined to depart from the properties or qualities of the parent, than most others of our improved fruits. In America, they are in common and general cultivation. No trouble is bestowed in either layering (which is practicable), or budding them. Sowing a quantity of the stones, they are sure to pick out from among the seedlings as many good sorts as they may wish to cultivate: few of these may be exactly like the parent; some may be superior, but all are passable, especially if the young trees have been selected by a skilful hand; and this he is enabled to do, merely from the appearance of the wood and leaves. Many new sorts have lately been obtained and brought into notice in this country; and this facility of the peach to multiply its varieties will no doubt be taken advantage of by propagators.

Of the NECTARINE.—This, it is allowed by all writers, is certainly a child of cultivation: there being no wild plant from which it could be derived, except the almond. It is therefore a collateral branch with, or rather of, the peach: of this no better proof can be given, than the circumstance that nectarines are sometimes produced by a peach tree. It is, however, a variety, as distinct in appearance as in some other of its delectable qualities; and altogether a valuable acquisition, worthy its name, and a place in every collection.

Of the VINE.—The value and transcendant excellence of this foreign fruit is too well known to require any extended account in this paper; as a native of the southern verge of the northern temperate zone, it only requires its natural degree of heat to bring it to perfection. The growth is luxuriant, is fertile, easy of management, and as it requires support, obedient to the trainer's will. Many excellent varieties are in our stoves and vineries; differing in hardness, size of bunches, and in colour and flavour of fruit. These, it is likely, have been gained from seeds; and as its cultivation has been primæval with the inhabitants of the earth, no wonder it received, for its unequalled utility, their chiefest care. The favourite sorts are commonly propagated by layering, though it is capable of being both grafted and budded; and these latter modes are particularly useful where it is wished to have different sorts on the

same tree, either for the sake of variety, or, for what is much more valuable, *an extended vintage.* The acclimatizing, or enuring superior sorts to our seasons, is a desideratum among horticulturists at this time; and there cannot be a more laudable object.

That the climate of this country has undergone a considerable change within the last hundred years, is allowed by all who have considered the subject; and nothing furnishes a more convincing proof of this, than the history of the vine. Previous to the reign of Henry VIII., every abbey and monastery had its vineyard. In the rent-rolls of church property in those days, and long afterwards, considerable quantities of grapes were paid as tithe; and the vestiges of some of those vineyards remain to this day. They were usually placed on the south side of a hill, in a light dry soil, having the surface covered with sand; the vines being trained near the ground. But with such inclement and changeable springs, and long protracted winters, as have been experienced of late, even such frost as is seen at the moment I am writing (24th April), vines as standards in the open air, would be destroyed; or, at least, no dependence could be placed upon them for a crop. But vineyards in the country could neither be so profitable, nor are they so necessary as they were in those days; international intercourse is now more open, and corporations, whether religious or civil, can be supplied with grapes in any shape, and their precious juice in any quantity, at a cheaper rate than either home-grown or home-made. In their cultivation in this country, practitioners are more liable to err in planting them in too rich, than in too poor a soil; the first adds too much to their natural luxuriance of growth, and always reduces the flavour of the fruit.

Of the Mulberry.—This fruit has not been subjected to the operations and attention of the improver so much, perhaps, as it deserves; true, it has been planted against walls, and as espaliers; and in both places has done well. It may also be grafted, &c.; and whether, by the knowledge and means of improvement which we now possess, any further melioration can be made, is yet to be seen.

Of the Orange.—This endless family of fruits it is pro-

bable had the small but useful wild lime for its progenitor. The monstrous shaddock, citrons of all shapes and sizes, oranges and lemons, are all varieties, obtained in the course of long cultivation. The plant is kept in this country more for its evergreen and ever-fruitful beauty and fragrance, than for use; and although there are orangeries in some noblemen's and gentlemen's gardens, where they are cultivated to the greatest perfection, still they are only considered as curiosities; as the immense importations of the fruit from the continent render the cultivation here totally useless.

The same may be said of the pomegranate and olive; they may be both cultivated in this country, but their cost would be more than their worth.

Of the Fig.—This fruit has been long in our gardens; a very ancient one is still alive in the garden of one of the colleges at Oxford. In its native country it produces two crops in the year, and this property makes its management rather difficult in a country where it can but with difficulty be made to produce one; and especially when trained in the common way to a wall, where the crop is often sacrificed to the useless symmetry of the tree. But improvements have been successfully made to obviate such mismanagement both in pruning and training. It is impatient of frost, and requires protection during winter; and is also impatient of the knife, and more, perhaps, than any other tree, is disposed to form its own natural head. When kept in a glass case, either planted in the ground or in pots, it well repays the trouble bestowed upon it.

Of the Quince.—This fruit remains very steadily in character to what it has always been known to be; the taste is too austere to be used alone from the tree; but with other fruits in pastry, or in the shape of preserves or marmalade, it is useful. There seems to be a very fair opening for the improvement of this fruit by the proper means. It is an excellent stock for over-luxuriant growing pears.

Of the Medlar.—Two or three sorts of this tree are in cultivation: they are placed in the lowest grade of fruits; though, when they are perfectly mature, they are much relished by

some palates. The azarola, service, and two or three others used in the south of Europe, are not worth notice here.

Of the Filbert.—The common wild hazle of our hedges has been improved, by chance or cultivation, into the several varieties of red and white filberds and cob-nuts. Working them upon the hazle, or upon themselves, is necessary; because, it not only makes them more fruitful, but also brings them sooner into bearing.

Of the Walnut.—This nuciferous tree has been cultivated in England more for the value of the timber than for its fruit. Its fine flavoured nut is, however, an agreeable addition to the dessert, and is often preferred to more luscious articles. With wine, it is not only a suitable accompaniment, but also affords a kind of amusement in extracting the kernel. There are several varieties, differing chiefly in the size of the nut, from the diminutive ben-nut, to the large or double French sort. The only improvement which can be expected in this, is a hardier sort which would be less susceptible of damage from frost.

Of the Chestnut.—The description of the walnut may be applied to this, as they are natives of the same climate; and their flowers are alike impatient of frost. The fruit of this is, however, inferior to that of the walnut, and seldom arrives at the same degree of perfection. The tree grows to a great size, and is one of the most valuable of our forest trees. In "days of yore," it must have been much more plentiful in this country, or more plentifully imported, than it now is; as the principal timbers of abbeys, cathedrals, and other ancient buildings, are chiefly formed of it: being equally durable as the oak, which it so much resembles, that they can hardly be distinguished from each other, but by the test of the wet edge of a chissel being stained by the oak, and not at all by the chestnut. Arborists have suggested the possibility of procuring from this, as well as from its congener the beech, larger fruit, by sowing only the largest kinds of nuts which can be selected.

Of the Melon and Cucumber.—These exotic fruits are extensively cultivated; the latter takes various shapes in our

bills of fare; the former is more a luxury than a fruit for general use; their culture on hot-beds forms a material branch of modern gardening, and with that of the gourd, pumpkin, squash, vegetable marrow, &c., is well known.

Of the Pine-Apple.—This sovereign of fruits is, and can only be, in this country, an appendage to opulence and rank. Several varieties are cultivated in our forcing-stoves, and grace the tables of the rich, and in as great perfection as they can be had between the tropics. In their wild state, they affect the sides of rivulets, and often under the shade of lofty trees; but are of inferior flavour, unless the weather is very dry when they are ripening off; and when cultivated, they receive little or no water during the last stage of their growth.

As the introduction of other intertropical fruits begins to interest the admirers of such productions; and as trials may be made without much additional expense in our stoves appropriated to botanical collections; it may be necessary here to mention the names of a few of them, for the information of those who have the means and the desire of making such trials.

Of the banana, or plantain. This has been fruited in Britain nearly forty years ago, and several times since. When the pinery or stove is lofty enough, they are a magnificent plant, and in appearance far surpassing what they are in their natural place in the open air; for then the winds tear their ample leaves to pieces, and give the plant a most unsightly appearance.

The mangoe has been also fruited both in France and England: there are several much improved varieties of it in Jamaica; and if a few *worked plants* could be had from thence, and a stove, or part of a stove, fitted up for them, there can be no doubt but that the mangoe may be had on the table as easily as the pine-apple.

The mangosteen is one of the most delicious fruits in the world. It has been fruited, according to report, in France. It requires the same treatment as the mangoe. To these may be added the jambosteen, rambosteen, and decku; they are natives of the oriental archipelago, and when obtained, might be cultivated along with the preceding. They are all superior fruits.

The following list are inferior compared with the others, but

are objects of curiosity, as novelties; and therefore deserve a trial, viz.

Custard-apple, two sorts.

Bread-fruit, the cut-leaved.

Do. the entire leaved, or jack-fruit.

Papaw. N.B., a male and female plant should be placed together.

Guava and granadilla.

Litchee, with its varieties, the whongee, ayee, and chuchee . . . } Chinese fruits.

Loungan, sour and sweet . . }

Loquat . . . . . }

Cocoa nut }

Date, and } supplied as articles of commerce.

Tamarind }

I have only to repeat that it is necessary, in trying to bring tropical trees into bearing, they be grafted or budded on themselves, or on any congenial stock, (such as the rose-apple, perhaps,) in order that they may be brought into a fruitful habit, more expeditiously. The plantain, papaw, and granadilla are excepted. All botanical names and physiological terms have been intentionally omitted. J. M.

Chelsea, April 28th, 1827.

The Gold-Headed Cane. Murray. 1827. Small 8vo. pp. 179.

Some of our readers will be surprised at the apparent incongruity of this title with the usual materials of our Journal, and may imagine, for a moment, that we are ambitious of variegating the level scenery of science with some undulating forms of romance. The apprehension is groundless,—since, beneath this sportive mask are concealed some features of sterling and prominent characters; eyes, full of meaning, which have been keenly fixed on many remarkable events; and lips that, in their time, have uttered not a little wisdom. The author, indeed, has not followed the prevailing mode of deluding the unsuspicious reader by a certain majestic importance of front: he conducts us through a humble gateway into elegant apartments, and familiar converse with

the great and learned,—while, too often, alas! in the course of our literary rambles, are we tempted by the columns of some prouder portico, to stumble over paltry furniture, or to pine in ungenial society.

The Gold-Headed Cane, then, which is the hero of this little volume, was once grasped by the well-curved palms of Radcliffe, Mead, Askew, Pitcairn, and Baillie,—but is now condemned to the obscurity of dust in a corner of the new College of Physicians. Mortified by the disuse into which this ancient appendage of the art of healing has fallen, while the wig of the jurist still retains its honours, he consoles his retirement by reviewing the transactions of which he had been a party while sustaining the weight of his former illustrious patrons. We are thus presented with an entertaining picture of several of the most celebrated physicians of the last hundred and fifty years,—and of not a few of the political or literary contemporaries with whom they were occasionally placed in contact. The outline is medical; but some traits of general life are intermingled—some fleeting tints of society are scattered throughout, to complete the design. We are introduced behind the scenes, to glance over the actors and the audience of other times: the general impression is analogous to that produced by the Private Memoirs which the French have so voluminously multiplied, and which we are at present so industriously emulating,—excepting, always, that our author has offered not a single sacrifice on the altar of party, prejudice, or pique. The urbanity of tone which moderates the whole strain of the composition, distinguishes it from every collection of anecdotes with which we are acquainted. An inventive raillery might, on this ground, have found ample room to amuse the living at the expense of the departed; but the author has wisely avoided this cheap avenue to popularity, and has clothed his personages in their genuine vestments,—in that easy dress which human nature usually wears in every age and place, never perfectly symmetrical and dignified, and never entirely disproportionate and mean.

A chasm existed in the medical biography of England, which the present volume fills up with taste and accuracy, if not with minuteness. The admirable History of Freind extends no farther than to the foundation of the College of Physicians of London. Dr. Aikin, while engaged in the practice of surgery, produced an elaborate, but neglected, volume, which commences with the life of Richardus Anglicus, and terminates with that of Harvey and of Glisson.

He pursued the track of his eminent predecessor with much success: in the happy association of easy elegance of style, of entertaining narrative, and of profound criticism, these authors maintain the first place amongst their rivals in the same career, and have distanced both Cabanis and Sprengel. But we may remark, that the obscure fate which the Memoirs of Aikin have incurred,—the rarity of the History of Freind, and the long interval which has elapsed since a new edition has been demanded, afford a severe evidence of the diminished value which the medical public of this country attach to the annals and literature of their profession. The press of Germany and France, on the contrary, is daily teeming with books of this description,—an abundance whose origin is partly to be traced to the larger proportion of regularly organised medical schools, where the ablest professors are contented, on the recompense of a scanty salary, to devote an entire life to the cultivation and diffusion of medical erudition. In most of the German Universities a professor is found, whose sole or principal office is to illustrate the history of medicine. The present ingenious volume adds a new link to the chain commenced by Freind, and continued by Aikin: less practical in its details, it is more calculated to attract the general reader; and the faithful record which it exhibits of the brilliant progress, the public and private virtues, and the patriotic influence of a long series of physicians, must tend to impress on his mind an increased respect for the profession at large, and a more attentive consideration of its interests and importance.

We are first ushered into the dignified presence of Radcliffe, and accompany him on his visits to King William and Queen Mary, as well as in less awful peregrinations, where he sometimes drew on the pharmacopœia of his bold and ready humour for the most efficient remedies. His prognosis was singular in confidence and success. He was once sent for to attend the Duke of Beaufort, at Bodminton, but declared to the gentleman who brought the summons, that his presence was unnecessary, since the duke had died at a particular hour on the preceding day—an assertion which was verified by the messenger on his return. Radcliffe had occasion to see the King, after his return from the continent in 1697, and insinuated to him that he ought not to indulge the hope of a speedy recovery; but added, "if your majesty will forbear making long visits to the Earl of Bradford, (where, to tell the truth, the King was wont to drink very hard,) I'll engage to make you live three or four years longer;

but beyond that time no physic can protract your Majesty's existence." This daring prediction was accomplished in 1702, when a curious glimpse is afforded to us of the practice of that period, and of Dutch, French, and English art vainly struggling to prolong the life of a monarch.

"In addition to many other infirmities under which the King laboured, he was troubled with boils that formed in different parts of his body; and for these Bidloo directed that his feet and legs should be rubbed night and morning with flannel covered with powder of crabs' eyes, flour, and cummin-seed. As to diet, the doctor was exceedingly indulgent, allowing his master to drink cider, ale—in short, all sorts of strong beer; and to take crude aliments before going to bed. It was in vain that Doctors Hutton, Millington, Blakemore, and Laurence, remonstrated. On the King's return to Hampton Court, the dropsical swelling of the inferior extremities extended upwards, for which Bidloo prescribed a vapour-bath, and inclosed the legs of the patient in a wooden box constructed for that purpose. In a constitution so weak, which this treatment was reported to have still more debilitated, an accident was likely to prove fatal. On the 27th of February, 1702-3, while hunting, the King fell from his horse, and broke his right clavicle near the acromion. This occurred in the neighbourhood of Hampton Court; but the French surgeon Ronjat was at hand, and soon reduced the fracture. But when he wanted to bleed his Majesty, a new obstacle arose, for it was necessary not only to have the sanction of some one of the court physicians, but also the authority of the privy-council, for the performance of that operation.——Notwithstanding the necessity and advantage of rest, the King persisted in his wish to return to Kensington, where he arrived between nine and ten o'clock in the evening: here a discussion arose between Bidloo and the surgeon, as to whether there had been really any fracture or not. Ronjat stoutly maintained the affirmative; the Dutch doctor as stoutly denied it. This point was, however, at length settled, when a new difference of opinion occurred as to the mode of applying the bandages. Bidloo wished himself to apply them, but the surgeon said no;—'you are here either in the character of a physician or in that of a surgeon: if the former, you have nothing to do with bandages; if the latter, "c'est moi qui suis le premier chirurgien du Roi."' After the death of the King, a paper war took place . . . . . on the one hand Bidloo put forth a pamphlet, published at Leyden, written in Low Dutch, in order, as his enemies said, that few might read it in this country: the year after, M. Ronjat entered the field in a French reply."—p. 24—26.

The cane is next carried by Mead,—perhaps the greatest physician who ever flourished. The students who feel a

generous pride in their profession will pause with pleasure over the numerous and shining passages which here delineate his habits, his qualities, and his conversation. A very good account is given of the precautions which he recommended against the plague, afterwards adopted by an act of parliament;—of his experiments on, and recommendation of, inoculation;—and of his introduction of the plan of bandaging after the operation of tapping. In a trying moment, Mead evinced that his heart was even more exalted than his understanding. Dr. Freind had been committed to the Tower, in consequence of his warm and able opposition in parliament (the habeas corpus act being at that time suspended); and, during several months of confinement, his valuable practice fell chiefly into the hands of his rival Mead. What was the conduct of this admirable man? After having employed every effort to release the prisoner,—he was at length summoned to relieve an indisposition of Walpole, but refused to prescribe for the minister unless Freind was set at liberty; and, having accomplished this object, presented him with five thousand guineas, the fruit of his attendance on the patients of the captive. No physician had ever obtained so brilliant a recompense of his labours;—he generally received between five and six thousand pounds per annum—a sum which must be at least quadrupled in order to approach the present value of money. But his reputation, both domestic and foreign, exceeded his revenue: *Blumenbach*—no vulgar judge—thus designates him, thirty years after his death:—"Inter Anglos hujus periodi maxime præfulgit, vir undique doctissimus, et qui innumeris modis de augmento scientiarum meruit." Such opportunities as he enjoyed will probably never again occur to an individual, because the domains of medicine are more numerously, more ably, more equally cultivated than in his time;—but we may all aspire to imitate, in various shades of capacity, his generous patronage of obscure merit, his disregard of selfish accumulation, his frank commerce with his scientific brethren, his kindness to the humble client, and his dignified deportment towards the titled one.

We have taken pains, in some instances, to verify the authenticity of anecdotes and facts recorded by our biographer, and have uniformly found reason to commend his fidelity. As a specimen we may mention that, in the account of the interview at the Tower (p. 75), the remark with which Freind receives Mead, is an exact translation of the postscript of the Letter which he addressed to him during his detention:—"Ego scribo hoc, cum permissione atque

etiam indulgentiâ Præfecti, in præsentiâ Warderi, qui cum in scribendo me non multum adjuvit, facit, quod tibi gratum fore reor, ne longior sim." (J. F. ad R. M. De quibusdam Variolorum Generibus.)

England is richer than any other country in anatomical preparations,—and our anatomists have always maintained a high reputation for their skill in this department. In 1823, the late Earl of Winchilsea presented to the College of Physicians some curious specimens which belonged to his ancestor Dr. Harvey.—

"The preparations themselves consist of six tables, or boards, upon which are spread the different nerves and blood-vessels, carefully dissected out of the body: in one of them, the semilunar valves of the aorta are distinctly to be seen. When Harvey delivered his Lumleian Lectures, he may frequently have exhibited these preparations, and, by their help, explained some points of his new doctrine of the circulation of the blood. They were most probably made by Harvey himself; and he might have learned the art in Italy, for he studied at Padua in 1602. A few years afterwards, on his return to England, he was appointed anatomical and surgical lecturer to the College of Physicians, and, in 1616, read a course of lectures there, of which the original manuscripts are preserved in the British Museum. In the College of Surgeons are some preparations, similar to those of Harvey, which originally belonged to the Museum of the Royal Society, kept at Gresham College. They were the generous gift of John Evelyn, Esquire, who bought them at Padua, where he saw them, with great industry and exactness, (according to the best method then used,) taken out of the body of a man, and very curiously spread upon four large tables. They were the work of Fabricius Bartoletus, then Veslingius's assistant there, and afterwards physician to the King of Poland. . . . . The history of the art would be a subject of curious investigation. In the Philosophical Transactions for May, 1666, Mr. Boyle mentions a method he had invented of preserving or embalming the embryo of a chick, in a glass filled with spirit of wine, to which he sometimes added a little sal ammoniac, as he observed it never coagulated the spirit of wine. Ruysch, the professor at Amsterdam, if not the discoverer of the use of injections, for the display of vascular and other structures, contributed, together with the suggestions of De Graaf and Swammerdam, by his own ingenuity and industry, to introduce that important practice among anatomists. His museum became ultimately the most magnificent that any private individual had ever, at that time, accumulated. . . . . It was purchased in 1717, by the Czar Peter the Great, for thirty thousand florins, and sent to Petersburg. Dr. Frank Nicholls, who married a daughter of Mead's, was the inventor of corroded anatomical pre-

parations. He was at one time professor of anatomy at Oxford, and author of a treatise *De Animâ Medicâ*."—(p. 102—104.)

We pass over some amusing anecdotes in the chapters dedicated to Askew and Pitcairn, and shall approach the death-bed of the latter, of whom a most amiable portrait is previously drawn.

"Pitcairn had never published anything himself; but the peculiar and melancholy privilege was reserved for him, to enlighten his profession in the very act of dying. On the thirteenth of April, he complained of a soreness in his throat; which, however, he thought so lightly of, that he continued his professional visits during that and the two following days. In the night of the fifteenth, his throat became worse, in consequence of which he was copiously bled, at his own desire, and had a large blister applied over the throat. On the evening of the sixteenth, Dr. Baillie called upon him accidentally, . . . and even then observed no symptom that indicated danger. But the disease advanced in the course of that night, and a number of leeches were applied to the throat early in the morning. At eleven o'clock in the forenoon, Dr. Baillie again saw him. His countenance was now sunk, his pulse feeble and unequal, his breathing laborious, and his voice nearly gone. In this lamentable state, he wrote upon a piece of paper, that he conceived his windpipe to be the principal seat of his complaint, and that this was the croup. The tonsils were punctured, some blood obtained, and a little relief appeared to have been derived from the operation. Between four and five o'clock in the afternoon his situation seemed considerably improved; but soon afterwards a slight drowsiness came on. At eight, the patient's breathing became suddenly more difficult, and in a few minutes he was dead. This was the first case of this peculiar affection of the throat that has been distinctly recognized and described. It was an inflammation of the larynx. . ."—(p. 148.)

Dr. Baillie was remarkable for clearness of expression in the delivery of Lectures, and conveyed his information to his pupils in the most simple language:—

"For this talent he was greatly indebted to the assiduous instruction of his uncle, who spared no pains in cultivating in his young pupil a habit of ready and exact explanation; and was accustomed to teach him in this manner:—'Matthew, do you know anything of to-day's lecture?' demanded Dr. Hunter of his nephew.—'Yes, sir, I hope I do.' 'Well, then, demonstrate to me.'—'I will go and fetch the preparation, sir.' 'Oh, no, Matthew, if you know the subject really, you will know it whether the preparation be absent or present.' Dr. Hunter then stood with his back to the fire, and his nephew demonstrated."—(p. 153.)

We shall close our quotations with some detached sentences from the observations with which the President accom-

panied his announcement of the bequests of Dr. Baillie to the College.

"His purse and his personal services were always at the command of those who could prefer a proper claim to them; and every branch of the profession met with equal attention. . . . . In consultation he was liberal and candid in the highest degree; and so industriously gave credit to the previous treatment of the patient (if he could approve of it), that the physician who called him in never failed to find himself in the same possession of the good opinion of the family as he was before the circumstances of the case had made a consultation necessary. His manner of explaining the disease, and the remedies recommended, was peculiar to himself, and singularly happy. It was a short, compressed lecture. . . . . Before his time it was not usual for a physician to do much more than prescribe remedies for the malady, and to encourage the patient. . . . . But as the assumed gravity and outward signs of the profession were now considered obsolete customs, and were, by general consent, laid aside by the physicians, and as a more curious anxiety began to be observed on the part of the patient, to learn everything connected with his complaint, arising naturally from the improved state of general knowledge, a different conduct became necessary in the sick room. But justice cannot be done to his medical character, unless that important feature in it which appeared in every part of his conduct and demeanour, his religious principle, be distinctly stated and recognized."—(p. 170—176.)

The wood-engravings with which this volume is sprinkled, are not without interest to the physiognomist and the antiquarian. We hope, ere long, again to encounter Dr. Macmichael in the field of anecdote, where he is so active and cheerful a sportsman;—and that, in a future edition, he will find time to double the number of his pages, and to wander more freely over the deserted, but fertile, territory of medical antiquities. The last hundred years may procure ample exercise for his remark, either in watching the movements of the regular service, or in tracing to their secret haunts the guerrilla corps of Magnetism and Metallic Tractor, the poachers on Longevity, or the smugglers of the Tea of Life. His reminiscences of the medical world will be equally pleasing to the veteran who is reposing on his oar, and instructive to the younger mariner who is about to launch his skiff. England has been stigmatized as the paradise of quackery,—with more justice she might be styled the paradise of medicine; and it would be a mark of real ingratitude, if a country, where fame and fortune so largely remunerate the professors, could find no one ambitious of celebrating the progress of the science.

Revised Arrangement of the Ornithological System of Cuvier and Dumeril, with reference only to the Genera and Species of the British Islands.

Birds are vertebral and oviparous animals, with lungs and warm blood, producing their young enveloped in a calcareous shell, requiring a heat of 100° Fahrenheit to mature their formation. In other respects there is a great analogy with the mammiferous class of animals.

Their bones are generally hollow, containing air; their lungs are attached to the ribs, perforated externally, by which this fluid is distributed through every part of the body, but chiefly into two large membranaceous vessels, or sacs, situated in the thorax and abdomen, by which the body is much enlarged, and rendered considerably lighter. This provision of air affords, in some degree, an additional power of respiration and vigour. It is even supposed that the increased temperature which birds acquire during incubation, and the extensive flights they are enabled to undertake in their periodical migrations, depend much upon the action of air on the blood. It has been ascertained that two sparrows consumed, in a given time, as much air as a guinea-pig.

They sleep resting on one foot, the weight of the body being sufficient to act upon a series of muscles, extending through the thigh to the extremity of the toes, which, in proportion to the pressure of the body, contract and adhere to the perch on which they rest.

The neck is amply supplied with vertebræ moving in all directions, but in the back-bone they are connected and motionless.

The breast-bone, with which the muscles of the wings are connected, is of considerable size and strength; it is composed of five parts; the degree of ossification in the posterior lateral pieces is proportioned to the power of flight in each species.

The wing is divided into three principal parts,—the shoulder *(brachium)*, the arm *(cubitus)*, the hand *(carpus)*. The feathers on the hand are always ten in number, called *Primary;* those on the arm, *Secondary;* those on the shoulder, *Scapulary;* those on a small projection from the hand form what is called the bastard wing.

The tail usually consists of twelve feathers, sometimes ten or fourteen, and in the gallinaceous order, eighteen.

The senses of touch and taste must be very weak, the claws and feet being covered with a hard skin, approaching in some species to a horny or scaly substance; but the deficiency in these two senses is compensated by great acuteness in the remaining three.

The eye is furnished with a thin membranaceous veil, which the bird can draw and remove at pleasure, easily discernible in the hawk tribe. Its probable use is to guard the eye in rapid flights, or to moderate the effect of light. By a peculiar additional mechanism, birds are also enabled to regulate the sight for near or distant vision.

The digestive organs merit particular notice. The throat terminates in a large membranaceous bag, called the crop*. Here the food is collected and softened, preparatory to its passing into the gizzard, where it is reduced to a pulp. This organ consists of two very powerful muscles, lined and covered with a strong tendinous coat, furrowed within. In rapacious birds, the gizzard is, however, wanting, their stomach being nearly allied to those of quadrupeds. The power of this organ almost exceeds belief, it having been proved by experiment, that very hard and sharp substances are unable to resist its action. It has been observed, that stones which birds are in the habit of swallowing are seldom known to pass with the fæces. They, therefore, probably contribute to their health by affording an additional supply of calcareous matter; and digestive organs of this singular character are consequently requisite for the purpose of pulverizing them sufficiently for admission into the system.

The channel conveying air to the breast is called the *Trachea*, and differs from that of other animals. The organ producing sound is situated immediately below the point where the trachea separates into two channels, and is called the *Larynx inferior;* so that a bird might emit its characteristic note after decapitation.

J. S.

* It is of so light and close a texture, that the crops of turkies are prepared and sold in London as small balloons; when filled with hydrogen gas they will rise with great rapidity, and retain the gas for a considerable time, particularly if slightly varnished.

BRITISH ORNITHOLOGY.

Birds are divided into Six Orders:*

- Number of back toes
  - Two and two before — Scansores, Climbers.
  - Only one, or none — Quite unconnected, beak and claws hooked — Accipitres, Rapacious.
  - The fore toe — Connected
    - Entirely by a wide membrane — Palmata, Web-footed.
    - To a certain degree by a slight membrane — All at the base — Gallinæ, Gallinaceous.
    - To a certain degree by a slight membrane — The two external only — Shank... Very long — Grallæ, Waders.
    - To a certain degree by a slight membrane — The two external only — Shank... Moderate length — Passerinæ, Passerine.

Order I. ACCIPITRES. *Rapacious.*

One back toe; those in front quite unconnected; beak and claws hooked. Of this order there are two subdivisions.

- Eyes...
  - Lateral, on one side of the head — Plumicolles.
  - In front — Nocturnæ.

Subdivision 1. Order I. *Rapacious.*

PLUMICOLLES.

Eyes lateral; base of the beak with a fleshy excrescence called the Cire.

- Wing feathers
  - 2d the longest; 1st nearly as long; Beak curved from base, and notched at each side, near the end — Falco, Falcon.
  - 1st very short; 4th usually longest beak
    - Long and straight from base, hooked at the end... — Aquila, Eagle.
    - Short and curved from base
      - Wings...
        - Longer, or equal to tails, which in kites are forked — Buteo, Buzzard kite.
        - Shorter than tails... — Accipitres, Hawk.

* The Orders are placed in the above succession on account of the natural distinctions specified; but they are usually enumerated as follows:—1. Rapacious; 2. Passerine, including some of the Picæ of Linnæus; 3. Climbers; 4. Gallinaceous; 5. Waders; 6. Webfooted.

Subdivision 2. Order II. *Rapacious.*

NOCTURNÆ.

Eyes very large, in front of a large head; beak short, hooked, covered at the base and over the nostrils with stiff bristles.

- Head ..... With tufts in form of horns ..... Horned owls.
- Head ..... Plain, without such tufts ..... Without horns.

Order II. PASSERES. *Passerine.*

One back toe; the two exterior in front connected; the shank of moderate height. Of this order there are six subdivisions.

- Beak ... Notched ..... Dentirostra *.
- Beak ... Without notches... Strong and solid ..... Strong, long, compressed... Plenirostra.
- Beak ... Without notches... Strong and solid ..... Rather bent, short, conic... Conirostra.
- Beak ... Without notches... Weak and flexible ... Short, and at the base..... Narrow, round ..... Subulirostra.
- Beak ... Without notches... Weak and flexible ... Short, and at the base..... Wide, flat ..... Planirostra.
- Beak ... Without notches... Weak and flexible ... Very long and narrow ..... Tenuirostra.

Subdivision 1 of Order II. *Passerine.*

DENTIROSTRA.

Beak notched on each side near the end.

Beak ... Straight	Strong, compressed, hooked at the end ...		Lanius,	Shrike.
Beak ... Straight	Flat, and bristled at the base		Muscicapæ,	Flycatcher.
Beak ... Bent	Nostrils	Hid in bristles	Cotinga,	Chatterer.
Beak ... Bent	Nostrils	Naked	Turdus,	Thrush.

Subdivision 2 of Order II. *Passerine.*

PLENIROSTRA.

Beak longish, straight without notches; solid, strong.

Nostrils	Naked; end of beak rather bent ..		Coracias,	Roller.
Nostrils	Hid in stiff bristles	Truncated	Corvus,	Crow.
Nostrils	Hid in stiff bristles	Elongated.....	Picus,	Pie.

* The Dentirostra of Cuvier are the Crenirostra of Dumeril.

Subdivision 3 of Order II. *Passerine.*

CONIROSTRA.

Beak conic, bent, and without notches.

Beak	Long			Sturnus,	Starling.
	Short, but very solid...	Very thick, large	Crossed	Loxia,	Crossbeak.
			Not crossed	Loxia,	Grosbeak.
		Moderate size; upper mandible ...	Covering inferior	Fringella,	Finch.
			Receiving inferior	Emberiza,	Bunting.

Subdivision 4 of Order II. *Passerine.*

SUBULIROSTRA.

Beak awl-shaped; short, weak, flexible, without notches; base narrow and round.

Beak in proportion to length of head ...	One fourth		Parus,	Titmouse.
	One half outer claw	Straight	Alauda,	Lark.
		Curved	Motacilla,	Wagtail.

Subdivision 5 of Order II. *Passerine.*

PLANIROSTRA.

Beak short, slender, without notches; wide and flat at the base.

Toes with.....	Claws similar	All four in front	Apus,	Swift.
		Three in front	Hirundo,	Swallow.
	Middle claw toothed on one side		Caprimulgus,	Goatsucker.

Subdivision 6 of Order II. *Passerine.*

TENUIROSTRA.

Beak long, narrow, without notches; often flexible.

Beak.....	Straight. The outer toe	Entirely united		Alcedo,	Kingfisher.
		Rather separated. Tongue long		Sitta,	Nuthatch.
	Bent. The outer toe	United up to the claw		Merops,	Bee-Eater.
		Rather separated. Tongue	Long	Certhia,	Creeper.
			Short, obtuse.....	Upupa,	Hoppoe.

Order III. SCANSORES. *Climbers.*

Feet with two toes in front, and two behind. Beak narrow at the base, not indented.

Beak — Straight. Tongue long. Tail — Short		Picus,	Woodpecker.
Beak — Straight. Tongue long. Tail — Long, squared		Yunx,	Wryneck.
Beak — Rather bent, mandible rounded; nostril prominent		Cuculus,	Cuckoo.

Order IV. GALLINÆ. *Gallinaceous.*

Front toes connected at the base by a short membrane. Of this Order there are two Subdivisions.

Beak — Straight at base, nostrils lodged in a soft protuberance	Peristera,	Dove tribe.
Beak — Short, strong, convex	Alectrides,	Poultry.

Subdivision 1 of Order IV. *Gallinaceous.*

PERISTERA.

Dove tribe; wings calculated for swift flight; body very little raised upon the legs; bill slender, weak, and straight at the base; nostrils lodged in a soft protuberance; toes divided to their origin. No genera.

Subdivision 2 of Order IV. *Gallinaceous.*

ALECTRIDES.

Poultry. Beak strong, short, conical, rather curved. Upper mandible arched, and shutting over edges of lower mandible.

Part of the head — Plumed or crested. Feet with	Three toes only	Otis,	Bustard.
Part of the head — Plumed or crested. Feet with	Four toes	Pavo,	Peacock.
Part of the head — Naked round the eyes only. Tail feathers	Equal, truncated	Tetrao,	Partridge.
Part of the head — Naked round the eyes only. Tail feathers	Prolonged, cuniform	Phasiana,	Pheasant.
Part of the head — Naked round the eyes and beak also	Horny crest on the head	Numidia,	Guinea-hen.
Part of the head — Naked round the eyes and beak also	No horned crest	Meleagris,	Turkey.

Order V. GRALLÆ. *Waders.*

Shanks very long; naked up to the thigh; outward toes united at the base. Of this Order there are four Subdivisions.

Beak — Blunt, obtuse — Flat very wide	Latirostra.
Beak — Blunt, obtuse — Round, cylindrical, slender	Tenuirostra.
Beak — Very sharp — Long, conic, cutting	Cultrirostra.
Beak — Very sharp — Narrow, compressed towards point	Pressirostra.

Subdivision 1. Order V. *Waders.*

LATIROSTRA.

Spoonbills. Beak blunt, broad, flat, thin, end widening into a rounded form. No genera.

Subdivision 2. Order V. *Waders.*

TENUIROSTRA.

Beaks flexible, long, cylindrical rounded, blunt, slender.

Beak	Much bent, three times longer than head, and curved	Upwards	Recurvirostra,	Avoset.
		Downwards	Numenius,	Curlew.
	Almost straight, as long as the Head. Back toe	Twice length of head at least; back toe long	Scolopax,	Woodcock.
		None *	Tringa, or Phalarope,	Sandpiper.
		Not resting on ground	Charadrius,	Plover.

Subdivision 3. Order V. *Waders.*

CULTRIROSTRA.

Beaks cutting, long, straight, conic, strong.

Beak	Straight pointed. Middle claw. Toothed within		Ardea,	Heron,
	Straight pointed. Middle claw. Not toothed. Head	Crested	Ciconia,	Stork.
		Plain	Grus,	Crane.
	Curved downwards		Tantalus,	Ibis.

Subdivision 4. Order V. *Waders.*

PRESSIROSTRA.

Narrow beaked. Beak pointed, narrow compressed, particularly at the point, and longer in depth than width.

Forehead	Feathered	Hind toe	Rallus,	Waterrail.
		No hind toe	Hamatopus,	Oyster Catcher.
	Bald; four toes; three anterior	Plain	Gallinule,	Water-hen.
		Lobated	Fulica,	Coot.

* This distinctive mark is not quite accurate; for, as Montague observes of the Snipe and Plover, the characteristic marks of the length of bill and want of back toe in some species, run so nearly into one another as scarcely to be divided.

Order VI. PALMATED. *Webfooted.*

Toes united by membranes; shanks not very long. Of this Order there are four Subdivisions.

- Front toes in number...
  - Four, or the back toe included in the same membrane ..... Pinnipedes.
  - Three. Beak...
    - Toothed or serrated ..... Sterrirostra.
    - Not toothed or serrated. Wing...
      - Very long ..... Longipennæ.
      - Very short ..... Brevipennæ.

Subdivision 1. Order VI. *Webfooted.*

PINNIPEDES.

The four toes united by the same membrane.

- Beak naked at its base, and rather .....
  - Hooked..... Pelicanus, Cormorant.
  - Very straight ..... Pelicanus, Gannet.

Subdivision 2. Order VI. *Webfooted.*

STERRIROSTRA.

The three front toes connected, or concealed in the web; wings long; beaks more or less serrated.

- Beak in length, exceeding width .....
  - Three times..... Anas, Ducks.
  - Five times ..... Anas, Morgansus.

Subdivision 3. Order VI. *Webfooted.*

LONGIPENNÆ.

Three toes united in one membrane; wings very long; beak not serrated.

- Beak .....
  - Straight, conic, angular, and equal ... . Sterna, Terns *.
  - Bent downwards.....
    - At the end a sort of hook or nail ..... Procellana, Petrels.
    - No hook or nail at the end ..... Larus, Gulls.

Subdivision 4. Order VI. *Webfooted.*

BREVIPENNÆ.

Wings very short; back toe very short and separate, or none; beak not toothed; feet placed very far behind.

- Toes.....
  - Semi-palmated; legs serrated .... Colymbus, Grebe.
  - Palmated.....
    - A back toe ..... Colymbus, Guillemot.
    - No back toe..... ..... Alca, Auk.

* Dumeril repeats the introduction of the Avoset immediately after the Terns, having previously classed it amongst the Tenuirostra of the Order Grallæ, forming its leading Genus Recurvirostra; in fact, its toes are connected by a web.

On Malaria.

[Communicated by J. Mac Culloch, M. D., F.R.S., &c. &c.]

It is a familiar fact, not merely to physicians, but to the people at large, that there is produced from marshy lands a peculiar substance, called *marsh miasma*, in physic, and to which Italy has given the term *Malaria;* from the effect of which on the human body, there are excited fevers of different characters, but generally divided under the two great leading heads of *intermittent* and *remittent*. What the chemical nature of this substance is, has not been discovered, though numerous experiments have been instituted for this purpose; yet, with respect to its properties, we know enough to believe that it is a compound gas, decomposable by certain agencies, and also capable of being conducted to certain distances from the place of its production, by the winds. Further, it is ascertained that it can be condensed or accumulated in particular places; that it can form a certain attachment to the soil, or to peculiar solid bodies, although this is not permanent, as happens with respect to the matter of contagions; and lastly, that it is particularly affected, both in respect to its propagation and production, by certain qualities of the atmosphere, consisting in its conditions as to temperature and hygrometrical moisture.

Thus, although ignorant of its nature, we are in possession of certain facts appertaining to its natural history, which we can convert to use in warding off its evil influences, or preventing the attacks of the diseases which it produces; while, if one class of precautions depend on this knowledge, the other consists in that remaining branch of its natural history, which relates to the causes through which it is, in the first instance, produced, and in the next, propagated.

Being, as I have remarked, produced by marshy lands very especially, it has been naturally concluded that its immediate cause was the mutual action of vegetables and water, though it has been disputed in what that precise action consisted. To ascertain this is, however, of some importance, as on the nature of that action must, in a great measure, depend under what particular circumstances it is produced; consequently, what

places ought to be suspected, and, in the view to prevention, avoided.

That it is the produce of vegetable decomposition cannot, apparently, be questioned, because, if it were extricated from living plants, it should be found in thousands of situations where it is unknown ; and that it belongs to wet soils, and also to hot countries, or to a high temperature, further proves that it depends on that process of decomposition which notedly occurs, in these circumstances, most rapidly and extensively. But a difficulty remains in attempting to determine what is the peculiar quality or stage of this decomposition under which it is produced; whether it is the process commonly called putrefaction, or some change among the elements of plants of a different nature.

One point is at least proved ; *viz.*, that it is not necessarily accompanied by any smell, or that it is not an odorous substance in itself. It may exist in abundance and virulence, without being sensible in this manner ; and hence it had been thought this arose from some mutual action of vegetables and water, which was independent of proper decomposition. This, however, must be an incorrect opinion ; or the living vegetable is not required for its production : since it is fully proved to be generated in abundance by vegetable fragments long dead and detached from the plant, by mere mud impregnated with unassignable vegetable matter, and also by such chemical substances as contain some of the elements of plants, without belonging to them ; of which sugar, as I shall presently notice, offers a conspicuous instance.

To ascertain these facts is important, because they form the groundwork for precautionary measures as to disease, as will speedily appear; while I may here make one remark as an example of the utility of this knowledge : it is, that while there is a popular fear with respect to putrid and stagnant waters, there is none respecting clear waters, under whatever form, producing living vegetables, and free of smell, while the danger, in reality, may be scarcely less in the one case than in the other.

Generally, therefore, we must conclude, that wherever plants in contact with water undergo decomposition, even as to their

expended and dead portions; or wherever fragments of plants are exposed to moisture; or wherever mud, impregnated with invisible or dissolved vegetable matter exists; or, finally, wherever any chemical compound of the vegetable elements is wetted or held in solution in water, there the poison in question may be, or will be produced, provided the temperature be sufficiently high: and it is this necessity for a certain temperature, which is the cause why that peculiar decomposition of vegetables which forms peat, does not produce malaria, however it may occur in peaty lands, because, generally speaking, the formation of peat is limited to climates or regions of a low temperature. The nature and cause of the exception now made will be explained immediately.

I need not attempt to inquire further into the nature of this decomposition, since there are no facts on which to found an inquiry. What we must conclude, is, that some of the vegetable elements are let loose and re-combined into a new gaseous compound, while experiments carefully conducted (as ought not to be doubted when Vauquelin has been engaged in them) have not detected even the presence of such a new substance in the atmosphere of marshes, far less its nature. It is, however, evident that it cannot be any of the hydrocarburetted or other chemical gases which it has, at different times, been supposed; while, remaining thus in darkness, the only test of its presence continues to be the effects as to disease which it produces on the human body.

But, as I remarked, we possess enough knowledge of its natural history to assist us in guarding ourselves against its effects; and as that question, utility, is the object of the present paper, I will proceed to describe as much of that as is admissible in so limited a space. To know all the classes or kinds of places which are capable of producing malaria, is the first and main object as to prevention: to know by what means it is propagated is the next, while the contingent circumstances necessary to its production, or to its effects on the body, consisting chiefly in temperature and moisture under various modes, will also demand attention. All these things being known, we have laid the preliminary knowledge, requisite to prevention, as far as that is in our power; without them, we are subject to those hazards

which arise from ignorance; and what those hazards and their effects are, cannot be very obscure, when it is computed, and, perhaps, truly, that half the entire mortality of the world is the consequence of malaria: of the fevers chiefly which are its produce.

But one circumstance requires to be premised; and this is, to prove that our own country can produce malaria, and is subject to its diseases: because, were this not the fact, the statements of this paper could possess but very little interest to general readers, whatever it might to physicians or philosophers. I think I shall prove that we are all interested in them, and that, also, to a considerable degree.

The chief disorders produced by malaria are intermittent fevers, and what are called by physicians, remittent fevers: writing to general readers, we should call them fevers simply: while further, it is usual among the people, for them to receive the term typhus; a name implying, strictly, contagious fever, and, as thus misapplied, a source, not of confusion merely, but of considerable evil in various ways. Not to enter into medical discussions, which would here be inadmissible, I must be content with simply saying what is easily proved; that the fevers, generally, which appear in summer, or from June to November, are of this class, and the produce of malaria; while, that agues, as they are popularly called, are produced by the same cause, is admitted on all hands.

Now, every one knows that such fevers do occur in summer, in many parts of England, and in certain districts, very conspicuously; and further, that while they have been peculiarly abundant and severe during the three or four last years, they were, in 1826, unusually destructive and numerous, having been the cause of a mortality, which, in England, must be deemed considerable. Every one also knows that agues occur in England, and very especially in certain districts, so well known as not to require being named: whence it cannot be doubted, that although, compared to France or Italy, we may be considered, comparatively, exempt from this cause of disease—we are very far indeed from not suffering by it; and that also to an extent, which, the more it is investigated, will appear the more serious.

But to put this out of all doubt, while it serves to prove also the power which we possess over this cause of mortality and disease, it is sufficient to review the history of our own country, or even of London, to be convinced that our climate is capable of producing malaria wherever the other necessary circumstances are present; and that, in as far as we are now exempted, it has been the consequence of an attention directed to these circumstances. Hence the reasons, and the inducements also, to further attention of the same nature, for which it is primarily necessary that we should possess an accurate knowledge of them.

How enormous and nearly incredible the diminution of agues and fevers has been in Lincolnshire, Essex, Kent, or generally in the fenny and marshy counties of England, since the agricultural improvements of those lands, is matter of notoriety, since much of it has even occurred in our own days: but even in London, on Burnett's authority, the intermittent raged like a plague in the reign of Mary; while, from Sydenham, Morton, and others, to whom we may add the testimony of Short, and more recently, the facts collected by Heberden, the years 1658, 1664, and those from 1667 to 1692, were years of a mortality from this cause, which, could that act now as it then did, would, probably, in any one year, destroy twenty or thirty thousand persons in London alone. And this national mortality from fevers, from the fevers of malaria, extended even down to the year 1729, according to Short's testimony—so recent are the improvements by which it has been diminished.

Here, then, it is at last proved, that the climate of England did not then exempt it from malaria; and as that climate remains the same, so must this poison be still produced; whatever exemption we do possess, being the consequence of having corrected or destroyed some of those places or soils by which it is generated; but we have not corrected them all, nor have we exterminated the disorders which we have diminished. This is the task which yet remains for us, and for that end it is, that I shall here endeavour to point out the circumstances which do produce malaria; since I have, I trust, proved that it does exist among us; too generally overlooked, and not unfrequently denied or even ridiculed, while the chief cause of this oversight

is the error to which I here allude of misstating the cause and nature of the summer fevers.

It is this error chiefly, and further, that of looking for the evidences of malaria in the production of intermittent solely, which prevents a correct judgment from being formed of the existence of this poison, and of the places producing it, though, in addition to the fevers of summer, I could, had I here sufficient space, and were this a proper place for it, easily show that a great many other disorders, often little suspected as consequences of malaria, are equally produced by it, and are equally the evidences of pernicious soils. But as I have no room for detailing all this evidence, I must be content with saying, that wherever I here mark any particular mode of land or water as productive of malaria, it is, that in some one or more cases of that variety, I have ascertained, by this very evidence, that it was present, and the cause of disease; while nearly all, perhaps the whole, are further admitted or proved to be truly pernicious, by the various French and Italian physicians, who have bestowed on this subject an attention which has been utterly withdrawn from it in England. And if this detail is general, rather than special, it is not for want of special instances and proofs in abundance, that I have not noticed these; but from the recollection, that to point out the insalubrity of certain spots, might injure the properties, by affecting the value of those, should that which is here adduced succeed in commanding belief.

It is plain, to commence, that there is no mystery or charm in the term, marsh; and that if such a tract of land can produce disease, it is because it contains vegetables growing and decomposing in a moist soil: this is the general analysis of the cause; and wherever, therefore, the same circumstances occur, that spot must be, as to all the objects in view, a marsh, or a source of malaria. A minuter analysis of the facts can scarcely be necessary, either as to the plants that grow in marshes, or to the mode in which the moisture exists: since, generally, the plants are similar, while all vegetables appear indiscriminately to produce the poison in question.

In the next place, a marsh, in the popular sense of the term, possesses a certain extent; and in the popular view also, this extent is judged necessary to the production of disease. Facts,

without number, prove that space, bulk, or extent is not essential; but that the smallest spot of the same character, or of the proper character, is sufficient to produce disease, though the extent of its influence must be expected to vary in consequence: and even without these facts, this would be judged true from the following argument.

It is ascertained that a marshy spot can influence to disease a place situated at a considerable distance from it; and this extent, in Italy, has been ascertained to amount to at least three miles. Now it is plain, that whatever the original body of malaria was, it must have been diluted by the atmosphere during such a course, to a very great degree, or that the quantity reaching any individual must be very minute. It must be indifferent, therefore, whether that minute quantity which thus acts, is a portion of a great body of the same substance, or whether it is the whole which was generated; and this must, in reality, be indifferent in any case, even without transportation. It is ascertained, that a single inspiration will produce the disease; and, therefore, whether in this case, or in that of dilution under transportation, the exposed person receives the whole produce of a square yard, or any other given space, or the same quantity out of a mass produced by twenty thousand or a million of square yards, the consequence will be the same. It is a mere question of arithmetic; and moreover, as the whole malaria of any marsh is the collective sum of the portions produced by each plant or fragment, whatever can act on the exposed individual will be sufficient to the effect, though it were the entire quantity in existence, or the single part will act as effectually as if it was accompanied by a thousand similar ones.

Thus it is of no consequence how small the marshy or pernicious spot is, provided it can act; and the only difference is, that a smaller spot will act on fewer persons; and also, that from the greater dilution which it must undergo on transportation, it must act at shorter distances, or may require absolute proximity or contact.

Having premised these two necessary considerations, it is sufficient now to inquire what are the modifications and forms of soil or water, or of certain spots and places generally, which

contain the elements of a marsh, or are distinguished by a vegetation accompanied by water. Whatever these may be, let their names be what they may, or their appearances as deceptive, they are at least suspicious, and in very many cases they will be found to be real causes of the diseases of malaria, wherever the temperature, or the season and climate, are favourable, while that these are so with us I have just shown.

In a general enumeration, I may name the following places or soils, before proceeding to such particulars as may be necessary in explanation. Besides proper marshes, fresh and salt, meadows, or wet pasture-lands generally, however situated, there are woods, coppices, and thickets, plashy and limited spots of grounds, sea walls and river embankments; and, as falling better under the head of water, lakes, rivers, and ponds, including mill-dams, ornamental waters, canals, and the pools of insignificant dimensions which occupy commons, gravel-pits, or other similar places, together with agricultural ditches and drains of all kinds. To these, I must also add, as coming under the heads of vegetable matter without vegetation, sewers and town-drains, dunghills, tide-harbours, flax-ponds, or other receptacles of putrefying vegetables, and lastly, at sea, bilge water.

With respect to the most common of causes, though it is often thought, in England, that salt marshes are not insalubrious, and, very particularly, that security is obtained whenever they are daily washed by the tide: an ample experience shows that they are as pernicious as fresh-water ones, and that a daily tide is no security. Some of the most poisonous tracts in Europe, indeed, in France, Spain, Italy, and Greece, are salt marshes; and of how little value or protection the sea is, is fully proved by the tide-rivers of the tropical climates, most notoriously the most pestilential places that exist, even in those destructive regions. Where such tracts are found in England I need not say, as they are always both obvious and well known.

It is not believed with us, that meadows can produce malaria: this is to be questioned, in the first place, on general principles, and from other European experience; and secondly, it can be proved by domestic experience, that they actually are the causes of fevers with us; and even independently of their ditches or drains, which, it must be observed, may often produce

disease, where the soil which they intersect may be incapable of that. In Italy, in France, in Spain, and in Greece, and almost invariably in the American states, they are not only causes of malaria, but among the most common ones. The great seats of that poison in those countries, are the vallies and plains which give passage to rivers, whether on entering the sea or otherwise; and out of these, there is a very small proportion marshy, compared to those which are merely meadow and pasture, and which, very often also, are the seats of cultivation. Did meadow lands not produce malaria, there could be few of those tracts abandoned in summer, compared to those which it is nearly impossible to inhabit after the heats are once established, while, to refer to individual places, would be to form a catalogue of no small length.

With respect to our own country, whoever shall please to inquire, will find that fevers occur in autumn, in those situations, particularly in hot seasons, when they are unknown in the drier lands, and more especially in the cultivated ones; nor can there be any reason to doubt that it should be so, when our climate has been proved capable, like the southern countries of Europe, of producing this poison from the same class of soils. And whoever also will inquire, will often find great surprise expressed by country practitioners, at the occurrence of fevers in rural and detached situations, where the commonly esteemed cause, contagion, cannot be suspected; while he who may pursue his inquiries on this principle, will be able to explain the causes without difficulty, from the presence of some spot or tract of this nature. And, I must add, that it is not even necessary that such pasture-lands should be flat meadows, as I could easily produce instances of endemic fevers, almost amounting to epidemics in the last summer, where the lands, being wet and poachy however, were not only elevated, but formed the declivities of hills.

There is one case of land, which, as to ourselves, is not worth noticing, as it does not occur among us.—I mean rice-fields; and if I here enumerate them, it is for the sake of preserving the integrity of the subject. But it gives me an opportunity of introducing one or two remarks which do concern us: the most important of which is, that meadow-lands will be pernicious in

summer, in proportion as they have been wet the preceding winter, and that the danger will be especially considerable, should they have been inundated, as is often the case in some of our flatter counties. The other is, that the act of breaking up such moist pasture-lands for cultivation, is often hazardous, as it is amply proved that malaria is thus produced or extricated in unusual quantity or virulence: this effect, in the hotter climates, being often such as to produce the almost immediate death of the labourers employed in it. Hence, to note a precaution as to the prevention of disease, for which no opportunity will here occur in noticing the ordinary remedies against malaria, it is a matter of prudence in all such cases to break up meadow-lands in winter, when malaria is not produced; or, if this cannot be done, in the early part of the summer, rather than, in spring or autumn, the two seasons in which the disorders produced by malaria are most active.

Woods, including coppices and thickets of whatever nature, comprise the next class of soils or places which I may notice as productive of malaria. Of the pernicious properties of those in warm climates, the evidence is too abundant to admit of doubt. In Africa, in the East Indies, and generally in the torrid climates, woods, forests, jungles, bamboo and reed thickets, and many more varieties than I need distinguish, are the most noted causes of the fevers which have so generally been the sources of disease, whether to permanent residents, or to armies engaged in such territories. They are, in fact, proverbial: the question is, whether they are similarly pernicious in our own country. On general principles, they ought to be so, though, in the same proportion as any other pernicious soils are, less extensively and severely; and that they are in reality injurious, is proved by a variety of experience. Sussex is one of the counties well known to be productive of autumnal fevers, and even of intermittents, if less popularly celebrated than the fens of Lincolnshire or the marshes of Essex. The inhabitants, at least, know this full well; and he who may examine into the localities, will easily find, that if they are not produced by the woods, there is no other cause; while, to confirm the reality of this cause, I could easily point out, in various parts of England, endemic diseases the produce of such woods and coppices, even in dis-

tricts that would be less suspected than Sussex. There cannot be much stronger language on this subject, than that of a medical friend in Wales; who, after a long residence in India, complains that he is still vexed by the jungle-fever, in the woody districts in which he practises, and who, without system on this subject, entertains no doubt that the cause lies in the close and damp coppices of his neighbourhood.

If I have noted, in the preceding catalogue, plashes of moist ground, meaning, under that term, to comprise all kinds of insignificant wet or marshy spots, I need not specify more minutely what they consist in; while their abundance every where renders all reference to places unnecessary. They are in reality marshes, though small ones; and if what I have said respecting space or bulk as to a marsh, is valid, then are they sufficient to produce disease, provided the proximity is sufficient. And if it would be fruitless to quote examples of disease produced by them, from the impossibility of reference, I must trust that my own experience of such events, combined with the general argument, will be a sufficient warranty.

The last variety of mere-land production of malaria, and requiring enumeration, comprises sea-walls and river-embankments; and the reasons for pointing it out are, chiefly, that being very often implicated with the remedy for marshy lands, it is the more apt to be unsuspected. Yet it is a species of land extremely productive of malaria; and not unfrequently the sole or principal cause of that which continues to be generated after the reformation of such lands, while seldom viewed as such. Such walls are, in truth, very generally petty marshes in themselves, as is easily seen along the banks of the Thames; and there is abundant proof that the reeds and other rank vegetation which occupy them, are a frequent source of the same disorders as those lands were accustomed to produce before their drainage and embankment. To know this cannot but fail to be useful: since it is possible, at least, to avoid such places, and often not difficult to manage those necessary works so as to diminish the hazard of evil.

I may pass to the localities or situations which have a nearer reference to water than to land. What relates to lakes is simple; and if it little concerns us, inasmuch as our own lakes

lie in a climate scarcely productive of malaria, the demonstration of their pernicious qualities in Italy and Switzerland is ample. The causes, immediately, are various; and I need not do more than name them. Their deltas, whether at the exits or entrances of the rivers, are meadows of the worst kind, because subject to inundation; their borders are often marshy; and, in addition, they frequently contain creeks or shallow places, productive of putrefying aquatic vegetables and stagnant water; while, in some situations, as diminished by the heat of summer, they often also expose offensive mud, generative of the same poison.

With respect to rivers, I may commence by one general remark. It is the observation of Volney, that there is not a river in America that does not produce malaria; and when, after enumerating in addition, vallies, woods, mill-ponds, meadows and lakes, he says that, through his whole circuit, he did not find a dozen houses without fevers; it is a testimony which is too pointed not to be quoted as to this and many other of the facts and places which have here been enumerated. And in France and Italy, the same opinion prevails; while, if we consider the reasons, it is apparent in the nature of their banks, producing a marshy vegetation; in the diminution, during summer, of their waters, exposing mud; in their casual inundations, rendering the inclosing lands wet; and in the general character of the soil and vegetation by which they are bounded. How these circumstances may exist and act in our own country, I must now leave to the judgment of others to determine; and it will be easy to see what variety of river may be suspicious or insalubrious. We need not look for danger in a rapid stream, a northern climate, or a hilly region; but the Ouse, the Lea, and similar rivers, cannot possibly be exempt, unless England claims an exemption on some principle different from that which exists in France and Italy.

But to this I must add one remark. It is an universal prejudice in England that there is no hazard from running water; as it is, that clear water cannot be insalubrious, and that stagnation and putrefaction are essential to the production of malaria. It would be fortunate, indeed, for France and Italy were this true; and fortunate for all the tropical climates, even

in a greater degree. I have already said that putrefaction is not necessary; while the fact that the most pernicious situations in the world are the banks of the tropical rivers, and the lands perpetually washed by a clear green sea, shows convincingly the little truth of this popular judgment. The rivers which I have just named, and many more among us, including the Thames in many places, are decidedly productive of disease, though running streams; but the evidences are too notorious everywhere to need urging. And therefore, when, with regard to mill-dams or other ponds, forming the class of waters next to be noticed, it is popularly said that they cannot be pernicious, because the water is constantly refreshed by a stream, it is a false as well as a dangerous opinion. Should such a receptacle admit a stream that replaced the whole water in a few hours, which is rarely the case, it would, even then, be no other than a slow river, and therefore no more free of danger than the worst of those.

In proportion as any species of soil or place is least suspected, and most common, it seems the more necessary to be careful in pointing it out and dwelling on it; and hence it is, that I am more desirous of urging the dangers arising from what will, I know, be despised, as I have experienced it to be. I allude here to a great variety of apparently insignificant waters, comprising mill-ponds, for various purposes, and on all scales; ornamental waters and fish-ponds, canals, and the endless pools and ponds found all over the country, in deserted gravel pits, and in various other casual situations not worth detailing.

It is always useful to examine any case of this nature by reducing it to its principles, or by a philosophical analysis, and to inquire whether it *ought not* to be insalubrious, before inquiring whether it actually is so. Now all these waters, be they what they may, include the elements of a marsh or of a water productive of malaria. Their margins are marshes, they contain aquatic and putrefying plants; and under the heat of summer, they disclose mud impregnated with vegetable matter. And this is true of the whole, whatever their extent or character may be: while, if plainly shown that bulk or space is not necessary even to a marsh, and that our climate is adequate to

the production of malaria, there is no reason, *à priori*, why they should not produce it.

Now let us see how the experience confirms this; and if it does, then are we in possession of a very extensive class of causes of disease, and of one which very especially we have in our power as to correction, which, and not the excitement of useless alarm, is the object of this paper.

Volney's testimony as to the pernicious nature of millponds is decided, as to America; and I have just quoted it. Monfalcon is equally decided as to the poisonous qualities of all such waters in France; and all the Italian writers agree that it is the same in their own country. And the evil does not depend on the name by which they are known, or upon their physiognomy, or uses, any more than it does on their dimensions. Let me mark a few of specific kinds which are condemned in foreign countries, as a guide to our judgments respecting our own.

There are many extensive districts in France occupied by ponds rather than lakes, maintained and farmed for an inland fishery, where the diseases from malaria prevail to such an extent, that the average of life does not exceed twenty years; where the people are decimated in every year; where absolute old age, in those who survive so long, takes place at forty; and where the aspect of twenty is that of fifty or sixty in countries such as our own; where even the children are diseased from their births, becoming subject to unceasing fevers if they live to seven, and thus continuing till the not far distant period of death arrives to terminate the literally long disease of life. I can here refer to Monfalcon for details that I dare not enlarge on, and for authority that no one will question.

Here is proof of the effect of such waters: and I will use the same writer's authority as to condemnation of all canals, ponds, and ornamental waters, of whatever nature; besides which, as to those who desire facts more specific, his word and that of other French physicians, will probably be taken for the fact, that the "canal" at Versailles, and the similar water at Chantilly, which are mere ponds scarcely exceeding that in St. James's Square, are the common causes of severe intermittent and remittent fevers.

I need not quote further foreign authorities. They who doubt, may not, perhaps, take my word for the fact; but while I could point out, in this country of ours, the same occurrences in abundance everywhere, from many waters of this nature, I shall prefer trusting to the examination of others hereafter: since a conviction derived from such a source will be of much more value than a belief founded on any evidence which I could produce; marked as that is, and easy as it would be, to point out the very places, often familiar to the inhabitants even of this capital.

But that I may make these remarks of the more utility, let me note one or two other particulars; since, as far as reformation or avoidance of the cause is attainable, the value of these remarks must depend on their specific nature.

If any one will be at the trouble of examining the condition of health and the characters of the disorders, and further, the time of the year, and the particular kind of seasons in which these prevail, as these relate to the inhabitants of such spots, more particularly if he will compare the results with what occurs among the same classes of people in dry situations, I cannot doubt that he will find everywhere such proofs as I might easily have brought before him, did I not dislike to name the places. Such are the houses of the opulent in such districts as those which border the Thames, the Ouse, or other slow rivers; houses where ornamental water has been introduced, and more especially, where these are confined by woods; ancient castles surrounded by moats, and, among the poorer classes, those placed by canals, mill-ponds, and in other analogous places: to which I might add, what, however, is rare in our own country, if common on the continent of Europe, fortifications; the diseases of which, when the ditch is wet, are notorious everywhere. And to these I may subjoin, what will excite more surprise from their apparent insignificance, the ponds of gravel pits, which will be very often found the causes of those fevers that occur in such situations.

What will be observed in all such cases, is, that the inhabitants, even where opulent, are subject to what is called vaguely, ill health; while, to use a common, if a vulgar phrase, they are places where "the apothecary is never out of the house."

And this ill health, where least marked, will be found to consist in a succession of petty and almost indescribable fevers, being, in reality, the very condition which torments the inhabitants of the pestilential parts of France and Italy, from their cradles to their graves, in a variety of painful disorders, including rheumatism and sciatica; and in what, if difficult to ascertain absolutely, is well known to those familiar with Italy and France, namely, visceral obstructions, and very particularly, disordered spleen: well marked to those who know these countries, in the peculiar sullen complexions and physiognomies of the individuals.

If such is the general character of this ill health, I might easily explain its action at greater length: while it will be remarked by any one who will make the inquiries, that, very frequently, whole families, which were formerly healthy, have become thus disordered on taking up such a situation; and that others have, reversely, recovered health by leaving it for a drier one. But if all this is too little marked to attract ordinary notice, particularly where the cause is unsuspected, (though even popular opinion agrees in the insalubrity of low and damp situations,) there is disease enough produced by all the class of places which I have been enumerating, to satisfy any one who is really acquainted with the disorders arising from malaria; I allude to the fevers, the dysenteries, and diarrhœas, and the choleras of autumn, which will be always found peculiarly attached to situations of this kind, and often in so marked a manner, that it is wonderful the fact has not excited attention long ago. Let any one attempt to recollect where it was, that, in the last summer, he has seen a whole street, a whole village, or the whole of the inhabitants of one house, suffering under fever, and he will as surely find that such street, village, or house, was situated near water in some shape; and that water, perhaps, not more than the pond belonging to the gold fishes, or the gravel-pit on the common.

But to remove one doubt which will naturally arise, let me make one remark on gravel-pits which will explain every other objection of a similar nature.

It is objected that waters or wet lands cannot be productive of malaria, because they do not excite ague; since this, un-

fortunately, in England, is too commonly reckoned the sole test of its presence. So far is this from being true, that in the most pestiferous parts of France and Italy, simple and original intermittent is a rare disease; so little is this the proof, and the sole one, of an insalubrious soil. To omit the various other disorders which malaria excites, it is the summer fevers which are the great tests, as they are the produce of this poison; and but for which, the malaria of Italy or any other pestilential country would be of little moment indeed. And it is in these, therefore, that we must chiefly look for the evidences of insalubrious spots among ourselves: while unfortunately, as they are too generally termed typhus with us, or else attributed to heat, fruit, or other fanciful causes, the real malaria which produced them is overlooked or denied, as the place by which it was generated remains unsuspected.

And though, with us, malaria does produce intermittents in spring, as it excites common or remittent fevers in autumn, there is a reason why the same waters or places do not excite the former disorders while they are the causes of the latter, and hence also why they remain unsuspected, or why their pernicious properties are denied: this is, that from the winter-rains, many of these receptacles of water, and gravel-pits very particularly, are filled to the margins, and are often also void of vegetation in spring: while, from the heats of summer, the shrinking of the water and the growth of aquatic vegetables converts them into so many petty marshes—often also exposing to the sun their noxious mud.

Thus have I explained what will be found applicable to numerous cases of a similar nature: while it must be remembered in addition, that heat is necessary to the generation of malaria, and therefore, that many places and lands will produce it in autumn, which would not have done so in spring. And if English physicians, and the people also, forget or deny that their autumnal and summer fevers are the produce of this poison, it is not wonderful that they doubt or deny its existence; while this dangerous and destructive error is confirmed by their similarly overlooking the visceral disorders, and the remainder of that long train of affections which arise from the same cause, and which are most fully proved to arise from it

by the unquestionable, and no less wide and severe experience of France and Italy.

To proceed with similarly unsuspected places, I may name all kinds of ditches and drains, as these are constructed for agricultural or other objects. These are, in their very essence, marshes, and often of the worst quality; since often in a state of putrefaction which is more rare in a real marsh of any extent. And very often they are the real cause of the fevers which continue in a country after those operations by which it has been drained; as would easily be proved by a corresponding examination. To know this fact, is, as in all other cases, to know the remedy, as far as that is in our power; avoidance as the one generally most easy, and further attention to keeping them clean and free from weeds,—an attention not less required for agricultural reasons. And that such is their power in exciting disease is proved, not merely by the experience of modern France and Italy, but by the remarkable fact that, in the times of ancient Rome, perfectly informed at all times on this subject, such regulations formed a part of the laws, both as to these receptacles, and as to all canals. I need scarcely say that, in such cases, a clean earthen bank is the easy remedy; as, in the case of ornamental waters, a stone margin is an effectual security—as far, at least, as the margins are concerned—since it is equally necessary to avoid the growth of sub-aquatic plants and the exposure of mud to the sun.

I have thus described as minutely as I dare within these narrow limits, the chief places or forms of land and water which, as producing a living vegetation, are the sources of malaria; reserving what relates to remedial processes, to a future paper on this subject; and I may now point out what remains, consisting in vegetable decomposition independently of vegetation.

Among these, the sewers of towns are assuredly to be reckoned: and as I can take a proof from France without the hazard of offence, it is sufficient to mention, that the Salpetrière was formerly subject to intermittent fevers, which attacked the inmates within the house; and that these being suspected to arise from the drains, these were closed up, with the immediate consequence of exterminating the disease. It

is to be suspected that the object of the great cloacæ of ancient Rome was the same, though their history has not reached us; and it would be easy to confirm the same opinion, by the history of the diseases of towns without end, and by that of their reforms on this point. Our own capital offers a striking example of the improvement of its health from this cause; while the history of Fleet-ditch is familiar. It had indeed been thought, and is still, that the fevers thus produced were typhus, or contagious fever: but while it is obvious that remittent or marsh-fever will explain the effects equally, so must it be remembered that these fevers occurred in summer; that they were peculiar to those particular vicinities; and that, from the reports of Sydenham and Morton, the fevers of London were of this very character. And the whole analogy of fevers produced by such repositories of putrefying vegetable matter, not altered as to its effects because mixed with animal matter, seems to prove, as clearly as anything can be proved, that these town-fevers, from this cause, are truly fevers from malaria, and not from contagion; while the deception which considered them such, from occurring in the same houses or streets especially, is easily explained. It is just the same now as to rural situations; and the errors are the same. The whole inmates of a house are affected with a fever, not because it is contagious, but because they have all been exposed to the same cause: while, unluckily, the occurrence of petechiæ and so forth, in bad cases, assists in perpetuating the error; as if this was not a common symptom in the marsh fevers of Italy, Holland, and France.

If I cannot prove, in this country, that the dunghills and pools so often found in farmyards and before the doors of cottages, are productive of malaria, I can at least quote the very decided and numerous testimonies of French physicians to this purpose: nor, indeed, would it often be easy to account for the fevers occurring in this country, and among the lower orders, without having recourse to this cause; while the whole analogy of the subject leaves no reason to doubt that it ought to be one.

And let me make one or two remarks here which apply to all the cases of mud and putrefaction of whatever nature. I have

already noticed that odour was not necessary to the existence of malaria; and the proof is, that while all such putrefying matters, even to marshes, possess as much smell in winter as in summer, the poison is produced only in hot weather. The other is, as to mud, that, without any visible vegetation, not only does its exposure to the sun in summer produce malaria and fevers, but that this can be disengaged, even through the water, provided that it is not very deep, and without absolute exposure. Thus, in the former case, have some of the most severe epidemics on record been produced by the stinking of lakes and rivers, and the exposure of their mud; as also happens often, and in the tropical climates very especially, from that brief exposure which follows the recess of the tide: while, in the West Indies, it is observed that fevers are invariably produced by certain pools, as soon as they are so far diminished in depth as to allow the air of the bottoms to escape through the water. Now, though our climate is less active in this evil, these are facts of value to us in the way of precaution, while they are among the least suspected causes of evil; since it has been ascertained, that fevers are produced where no other causes than these are present. And if I notice another fact, that, in the West Indies, the mere exposure of the naked soil, by removing stones which covered it, has produced a sudden and deadly fever; so have I, in this country, seen agues produced, immediately and decidedly, by the simple and little-suspected accident of the inundation and drying of a cellar.

But of fevers produced by the exposure of mud containing vegetable matter, the most frequent instances in our own country are those which occur in tide-harbours. In the warmer climates, this is as notorious and deadly a cause as any one that exists: and under this head, nothing can be more notorious than that which occurs in the tide-rivers of Africa and the East, or within the tropics generally; since this is the very analysis of those most pestilential of all spots, mangrove sea-beaches and rivers. Here, there is no proper vegetation in contact with the water, whether that be fresh or salt, as the stems of the trees rise out of the naked mud: while it is ascertained by endless experience, that such a river or beach is safe as long as the mud is covered, but that the fever seizes

on the boats' crews at the very instant of the recess of the tide; while its cause is rendered immediately sensible, by a peculiar earthy smell, well known, even many miles at sea, as brought off by the land-winds, and equally exciting fevers among the ships' crews.

Now, it is familiar that in our own country, fevers, and in summer as usual, are common in our sea-ports; while, from the persistent errors on this subject, always attributed to poverty, filth, confinement, and, of course, to contagion. That this is not the real cause, is proved partly by the season to which they are confined, if it was not by the fact that they are really not contagious, but mistaken for such by being attached to particular spots or houses; and partly by that other fact, that they will be found to appertain to the very kind of sea-ports which I have described, and not to occur in deep harbours or on clean and rocky shores. It is easy for any one to conjecture where they ought to happen; and any one who will inquire, will find that the experience corresponds to the theory as derived from the present view.

It is so important, for many reasons obvious to medical men, to distinguish between contagious fevers and those which are not such, and the evils which arise from misstating marsh-fever for typhus are so numerous, that I must be excused for urging this subject as to all these last cases, and as to one also of very great importance which is about to follow. And, independently of this medical importance, it is particularly necessary in all that relates to prevention: since, as long as these fevers shall be believed to be typhus, or to arise from any other cause than what they really do, it is plain that it will be impossible to apply any means of diminishing their numbers, whether by avoidance of the causes, or by directly removing or checking the influence of these. And I am sure that a more accurate attention, recently bestowed on fevers by practitioners, will confirm what I have said respecting the comparative rarity of proper contagious fevers; if these persons have not yet agreed to refer them to malaria, and have not learned to discover the various sources whence this is produced, and which I have here been attempting to point out and explain.

There is yet one important source of malaria, arising in rural situations, which I am bound to explain, and which will complete what I have to say of the causes of its diseases on shore: this is the steeping of flax and of hemp; though, with us, the latter is rare, and the former not very common; except in Ireland and in Scotland, where, from the nature of the climate, it is probably not very pernicious. In Germany, France, and Italy, this has been fully proved to be one of the most active and severe causes of fevers of this character; as, in New York and elsewhere, it has been similarly proved that the same consequences, productive also of mortal epidemics, have arisen from the putrefaction of coffee, potatoes, the refuse of the indigo manufacture, and other similar causes.

It remains to notice but one case, and that an unsuspected one,—a case also which will probably be disputed—while I cannot help thinking that it is one of very extensive and serious importance: this is, bilge-water. It is remarked, in the first place, that sugar-ships are peculiarly subject to bad fevers, and it is well known that the bilge-water from this substance is the most offensive that exists. Further, when a ship is what is termed sickly, at sea, it is always found that she is what is called a foul ship; of which it would be easy to produce specific examples in abundance from the history of the navy, were it not from the hazard of personal offence. And to confirm this, when it has been found that the same ship was sickly under one commander, and healthy under another, the cause has been equally traced to the neglect of cleanliness. And how destructive fevers have often been at sea, both in the naval and the commercial service, needs not be said; as it is unnecessary to point out the great loss of lives, the serious inconveniences, and, what is here especially necessary, the absence of every other cause of fever in such a situation as the open sea.

Now, if this class of fever is not the marsh fever, or the produce of a vegetable malaria, it would form a singular exception to all the analogies recently pointed out; while, further, if it is not so, whence does it occur in exact proportion to the presence of vegetable matter acted on by water, and whence especially does it occur in so marked a manner in sugar-ships?

But it is generally, perhaps always, considered as a contagious fever, and perhaps not unnaturally, under the neglect of this cause; since amidst such a crowd, it is an easy conclusion that the disease is communicated from one to another. Yet, if it were so, the cleanliness to which I have alluded would not be a prevention; and in which, were it a contagious fever, it would far more often spread to the people on shore, after reaching harbour, than it is found to do. That there are sometimes contagious fevers on shipboard is most certain; but such cases and their consequences must not be allowed to mislead us on so important point as this,—since this would be to deprive us of the means of prevention which are in our power, and which are as simple as they are efficacious.

But not to dwell any longer on this case, important as it is, I cannot help thinking, that when naval surgeons shall have duly considered this subject, they will learn to coincide in the same opinion; and if it should prove so, then may we learn to prevent at sea, that which has been a most grievous evil in more ways than it is necessary to point out. And if ships were to be duly fumigated with sulphurous acid before receiving fresh crews in harbour, with such further precautions as to the people themselves are well understood; and if, in addition, care was taken, daily, to wash the ship, so as that the pump should bring up water as clear as the sea without, it is almost impossible that diseases of this character should ever make their appearance, or impossible, at least, that they should ever again be a cause of serious suffering.

But I must terminate a paper which is rapidly exceeding its stipulated bounds; intending, in a future communication, to complete the subject, by describing what belongs to the propagation of malaria; what relates to the effects of climate; and what further may be done as concerns what is the real object of this paper,—the prevention or diminution of disease.

Transactions of the Horticultural Society of London, Vol. vi. Part 5.

This Part terminates the sixth volume of the Transactions of this Society; it contains the Preface, Index, and other materials for completing the volume, and a few miscellaneous papers, which will be noticed presently.

The Preface acquaints us with the progress of the Society since the termination of the fifth volume, and of the influence which its proceedings have exercised over similar institutions in various districts of the country. We are told that "from an anxious desire to hold out assistance and countenance to the efforts of all such bodies, wherever they may exist, the Council of this Society has lately determined to give annually to each local Horticultural Society whose affairs are directed by a president and council or committee, one of their large silver medals, to be awarded by the local society to some one individual in their own district whose skill in gardening shall appear to them to be most deserving of the distinction." This is a measure which we hope will conduce most materially to the fostering and encouraging of horticulture in every corner of the country. We are also informed, that of the expeditions undertaken at the expense of the Society in foreign countries, one is still in operation and the other has terminated. The former, under the charge of Mr. David Douglas, extracts from whose very curious Journal we laid before our readers in our last number, is expected to produce the most valuable additions to the ornamental plantations of Great Britain; and, to use the words of the preface, "it is hoped that from this expedition our gardens will become as well filled with the beautiful vegetation of the borders of the Columbus and of the Rocky Mountains, as it is already with that of the Ohio and Mississipi." Of that which has terminated under the direction of Mr. James M'Kar, who accompanied the Sandwich Island Chieftains on their return from this country, it is stated that this individual

"successively visited Rio Janeiro and St. Catharine's in Brazil, several ports on the coasts of Chili and the Sandwich Islands. On his return, he landed upon Albemarle Island, one of the Gallipsyos, touched at Chorillo Bay, on the coast of Peru, and revisited Chili, when he found an opportunity of reaching Santiago, and botanizing among the little-known region of the Cordilleras. Upon this occasion, Mr. M'Kar succeeded in procuring a supply of fresh nuts of the highly-prized Araucaria Pine, which arrived in England in a living state, and from which a considerable distribution has already been made by the Society. The collection of

trees and plants of all kinds was very large, and of the greatest value to science. To the Sandwich Islands this expedition has produced advantages which it is hoped that these countries will long continue to enjoy, and which may be no inconsiderable means of hastening the civilization of the natives. In addition to a large supply of European fruits and vegetables, Mr. M'Kar succeeded in transplanting safely most of the valuable productions of the same kind which are found in Brazil."

The Papers are these—

LXIV. *Notices of Communications to the Horticultural Society, between Jan.* 1, 1823, *and Jan.* 1, 1824, *of which separate accounts have not been published in the Transactions.*

This consists, as its title purports, of miscellaneous notices upon a great variety of Horticultural subjects, most of which are quite incapable of abridgement. Mr. Williams finds the Siberian crab the proper stock upon which to graft the much-valued golden pippin. Mr. Gregory represents a mode of preparing temporary protections to flued-walls, by building into the top of such walls wooden blocks of a particular construction, to which rafters can be easily attached when required. This is a very ingenious and useful plan, and of great importance in our northern counties. Mr. Lambert preserved nuts during the winter by placing them in a large brown earthenware pan, which, when filled, was placed in a deep hole in the garden; the top of the pan being covered with a flat piece of wood on which was put a heavy weight, the hole was filled with earth. By this method, nuts may be kept in a fresh state till the season of their maturity returns. Captain Rainier recommends medlar stocks for grafting pears upon. We suspect, however, that the objection to this is, that the pear will not, as the gardeners say, *stand* upon such a stock.

LXV. *Notice of the Siberian Bittersweet, a new and valuable Cider Apple.* By Thomas Andrew Knight, Esq., F.R.S., President.

This variety Mr. Knight considers of the greatest importance in cider counties. 'If the juice is made to evaporate in a moderately low temperature, it affords a large quantity of jelly of intense sweetness, which to my palate is extremely agreeable; and which might be applied to purposes similar to those to which the inspissated juice of the grape is applied in France.'

LXVI. *An account of two varieties of the Mango Fruit, which ripened in the garden of the Earl of Powis.* By Joseph Sabine, Esq.

A description of the two mangoes which, as we stated in our last number, were exhibited in the autumn of 1826 by

the Earl of Powis. It is illustrated by beautiful coloured figures of the two varieties.

LXVII. *An account of ten varieties of Persian Melons.* By Mr. John Lindley, F.L.S., &c. &c.

The varieties described in this paper are said to be for the most part of surpassing excellence, with great delicacy, but at the same time richness of flavour. There appears, however, to be greater difficulty in cultivating the melon of Persia than the less excellent but more robust varieties of Europe. The writer recommends a particular mode of treatment, and describes the ten varieties in detail.

LXVIII. *Report on new or remarkable Esculent Vegetables cultivated in the garden of the Horticultural Society, during the year terminating on the 31st of March,* 1826.

One of the most striking horticultural features of this age is the high degree of perfection to which esculent vegetables have been brought. The leeks, onions, cabbages, and kail of our forefathers exist, indeed, after their kind, but in so altered and improved a state, that we doubt whether their identity could be easily recognised if opportunities for actual comparison could be found. The object of this Report is to put the public in possession of information upon this head, derived in the garden, from an examination of the numerous collections that are yearly cultivated there. For an account of the numerous varieties mentioned in it, we must refer our readers to the paper itself. It is the first of a series of Reports on the subject, which are intended to be annually published.

On the Effect of certain Exercises on the Form, and their Influence in preventing or curing Distortions of the Spine.

A DISCUSSION on exercise comprehends several questions:—first, the necessity of exercise to the preservation of the health and form; secondly, how far those plans at present in fashion are calculated to improve the figure generally; thirdly, whether there is any danger in the performance of some of them; and lastly, how far they are beneficial in cases of distortion.

The importance of well-regulated exercise is acknowledged by all: for it is only by exercise that the several parts of the body

are fully developed, perfected, and preserved from falling into decay. In this climate we cannot enjoy the fresh air unless we, at the same time, keep our bodies in active exercise; and every one knows how much this conduces to the general activity of the frame, and assists in the operations of the digestive organs*. It is also the most effectual remedy for that morbid excess of nervous irritability which is sometimes observed even at an early age.

The works of Locke, of Addison, indeed of all the most eminent writers on education, contain admirable examples of the influences which activity of the body has over the operations of the mind. Even Hippocrates, who wrote a philippic against violent exercises, says, moderate exertion gives firmness and strength to the body, and vigour to the mind ; and in Pliny there is an observation to the same effect. " Mirum est ut animus agitatione motuque corporis excitetur." But the most forcible remark is made by Rousseau in his Emilius : " The weaker the body, the more it commands ; the stronger it is, the more it obeys ; the body must possess vigour to be obedient to the mind ; a good servant should be robust."

These views have not always been admitted ; for although it has been allowed, that the mental energies of the eastern nations correspond to the inactive and indolent life which they lead, it has been argued that the savage, who possesses bodily strength, agility, health, and all the animal faculties in greater perfection than man in the more advanced state of society, is but little removed above the brutes in regard to intellectual faculties. But this question has been so often and so well met, that it would be almost presuming, and certainly unnecessary,

* The following is rather a curious instance of the necessity of exercise; it is in the life of Eumenes, by Cornelius Nepos. " Hinc tamen, multis suis amissis, se expedivit, et in castellum Phrygiæ quod Nora appellatur, confugit. In quo cum consederetur, et vereretur ne uno loco manens equos militares perderet, quod spatium non esse agitandi; callidum ejus fuit inventum quemadmodum stans jumentum calefieri exercerique posset, quo libentius et cibo uteretur et a corporis motu non removeretur. Substringebat caput loro altius, quam ut prioribus pedibus plane terram posset attingere; deinde post verberibus cogebat exultare, et calces remittere, qui motus non minus sudorem excutiebat quam si in spatio decurreret. Quo factum est, quod omnibus mirabile est visum, ut jumenta ex castello educeret, quum complures menses in obsidione fuisset, ac si in campestribus ea locis habuisset."

to enter upon it. In truth a vigorous and healthy constitution, with all the bodily and animal powers in their full perfection, which can only be acquired and preserved by properly-regulated exercise, are quite compatible with the nobler and more distinguishing faculties of human nature.

The question of the necessity of exercise to the preservation of the form may be considered in a point of view which bears directly on the subject of distortion.

It is a law of the animal economy, "that the exercise of an organ is necessary not only to its perfection but even to its preservation." This is often exemplified by parts which are not kept in due activity; for if they are not exercised, they degenerate so as even to lose their peculiar characters. For example: as long as a joint is kept in activity, the apparatus continues perfect; but when the motion of the joint has ceased for some time, all its complex parts degenerate; their peculiar characters and structure disappear. In a joint which has become stiff or anchylosed, the character of every part is changed; —the bone is no longer hard, but softened and cellular; and the bursæ, the capsules, and the ligaments, form one indistinct mass of cellular membrane.

The converse of the above proposition holds—that, by exercise, the organs may be renovated, or even new ones may be formed. If a bone be dislocated, new cartilages, capsules, bursæ, sheaths, ligaments, all may be formed; and if these parts, constituting a new joint, be kept in activity, although they may not have the regularity of the apparatus of the original joint, they assume all the characters of the several parts.

A variety of facts show that not only the muscles and ligaments, but the bones, arteries, and nerves degenerate if they are not duly exercised; and it is even possible to distinguish between the bones of a person who has died in full vigour, and of those who have been long bedridden*. If a soldier in active

* A German author makes the following remarks on the skeletons still exposed on the field of battle at Murten, where Charles the Bold, with his Burgundians, fell a sacrifice to the patriotic valour of the Swiss. "The three hundred years during which they have been exposed, in great measure, to the open air, have little affected their prodigious firmness of structure. Instead of mouldering down in a few years of exposure, they were evidently firmer than in the recent subject. From rubbing

service receives a wound for which immediate amputation is necessary, or if the same operation be performed on a strong labourer while he is in full health and exercise, the bone is found hard and compact in structure. But if either the soldier or the hospital patient should, in consequence of the accident, be confined to bed for some time before the leg is amputated, the bone is found soft and spongy as that of a scrofulous person. Analogous facts may be observed in animals, and it is well known that the bones of the leg of the race-horse when he is in full vigour are as hard as ivory.

In no part of the system is this law better exemplified than in the history of the curvatures of the SPINE. The muscles of the spine, which, with the exception of those of the heart, are most constantly in action, and whose office is to support the vertebræ, may be so weakened by want of exercise as to become incapable of performing their function. When they are reduced to this state, the ligaments which bind the vertebræ together yield to the superincumbent weight; for the ligaments are affected in a secondary manner by the same causes which produce debility in the muscles; weakness of the muscles is therefore one of the most frequent forerunners of distortion.*

together in my box, they acquired here and there the polish of the enamel of the teeth. Out of the charnel-house at Murten, I selected skulls that attested the strength of the stroke by which, as appeared from the marks, the helmet was cleft, and which, being pierced in the orbits by the point of the spear, probably belonged to knights, since the spear would be directed against this as the most vulnerable part. I still possess these specimens, and I consider them as an incontrovertible answer to the question, how these knights could wear armour insupportable by the present race? They were more hardy and athletic than we are."—*Ebell.*

This is a more rational account of the prowess of our ancestors than the incredible histories which would make us believe that we had degenerated from their outward appearance and gigantic stature. The truth is, that the bones become closer in their texture, the whole frame of the body is more intimately knitted, and greatly strengthened, by that life of exertion and fatigue which our forefathers led.

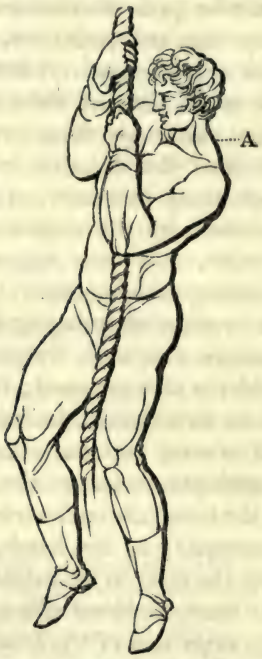
* This sentence must not be construed to imply that one set of muscles becomes weak, and the other so strong as to pull the spine to one side. When the muscles of the back have not been exercised, they become equally weak on both sides; the spine, being consequently not supported, must sink.

How far are Gymnastic Feats calculated to improve the form?

We have frequent opportunities of observing the actual deformity which arises from the disproportinate developement of any particular class of muscles. For example: the legs of those opera-dancers, who pride themselves on their powers of making extraordinary leaps and pirouettes, are almost Herculean, while their arms are comparatively diminutive. The same effects are produced on the form of those who practise horsemanship and tumbling, although these persons are sometimes better proportioned than the dancers, from their muscular system being more generally exercised; still their muscles are so unnaturally increased, by the violent exertions necessary to their feats of strength, that their appearance may also be considered as approaching to deformity.

Climbing a rope or pole, or swinging by the hands on a triangle, are the exercises now most frequently recommended. Mounting a rope-ladder is also proposed; few feats are so difficult to perform as this: for although the ladder hangs perpendicularly, being pliable in every part and not fixed at the bottom, when the climber attempts to mount the first steps they are pushed forward and the body falls backwards. The weight would now be sustained principally by the hands, if the climber did not attempt to keep the body in an extended line and nearly perpendicular. By these combined efforts, the exertion both to the muscles of the arms and of the loins is very great; this necessarily tends to strengthen these muscles. To this there may be no objection, as they are not seen; but in performing this exercise, and in climbing a rope, or pole, without using the knees as sailors do, a set of muscles are brought into action, the increased developement of which, instead of improving, tends to destroy the elegance of the figure. Is it desirable that a young lady should have a strong and short neck similar to that of a sailor-boy? for such will inevitably be her condition if she should ever attain any expertness in such exercises. When she attempts to raise herself on the ladder, or the pole, the head must, in some degree, be made a fixed point; and this is done by the action of the muscles between the head and the

shoulders*. These muscles, being then called into violent action to raise the body, are necessarily increased in power, and gradually become so much enlarged, as to give a peculiar roundness to the shoulders and back of the neck. This may be exemplified by the accompanying figure.



The jaws are also firmly clenched by their muscles, which, when increased in size, give the stringiness and harshness to

* Although it can scarcely be credited that mothers would permit their daughters to perform such an exercise as is represented here, it has of late been often practised, in the expectation that distortion would be cured by it. It is easy to show that such an exercise tends rather to increase than diminish the curve in the upper part of the spine. Although mothers could not be judges of this, they might have seen that there was some danger of the girl falling. About four months ago, a young lady in a family of distinction mounted the rope nearly as high as the ceiling, slipped, and fell on her back. She gradually recovered from the immediate shock, but she now suffers from a peculiar pain and weakness in the spine. Although it is to be hoped that this is merely symptomatic of an injury to the ligaments, still when the cases of the Count Lordat and others are recollected, we cannot help fearing that the membranes of the spinal marrow may become affected.

the expression, that may be observed in porters, or more particularly in the women who carry burdens suspended by a strap round the forehead; but the stringiness of the neck is from another cause, which will be explained in the observations on stooping.

The same objections apply to the plan of making those, who are slightly deformed, practise themselves in hanging by one or both arms.

The spine may appear more straight for the time; but the exertion to support the whole body is so great in the muscles of the shoulders and neck, that the effect is similar to that produced by climbing a rope or ladder. Indeed any one may be satisfied of this being the effect, by trying either of those exercises.*

Medical men are now often asked to give an opinion on the effect of certain postures and exercises, which have got the fine sounding name of Calisthenics.

Although it was not to be expected that those who proposed the exercises should be acquainted with the action of the several classes of muscles, it would not have been surprising if they had hit on some plans of exercising the muscles of the spine and ribs, that would have been useful in cases of distortion; but the principal exercises scarcely produce any effect on those muscles. Even some medical men who have proposed exercises of the arms for the cure of distortion, seem to have forgotten that many of the muscles of the shoulder have little or no effect on the spine or ribs. Unless exercises bring the muscles of the spine and ribs immediately into action, they have little effect on the position of the shoulders.

All that can be said in favour of the Calisthenics, is, that they are not attended with any danger, and that they may assist in improving the figure of a girl, who is merely sluggish and awkward; but the exercises proposed some years ago by Mr. Jenkins, who has been long known as a dancing-master, are

* Even the turning a wheel, which is considered one of the most simple and innocent modes of strengthening the muscles of the chest, may be injurious, unless it be judiciously managed. When the handle of the wheel is held by both hands, nearly in contact with each other, the effect of turning it, is rather to contract than dilate the area of the chest.

much better calculated for this purpose. He seems to understand, that the position of the head and of the shoulders depends on the form of the spine and ribs; and, therefore, instead of bringing only the muscles of the arms and legs into action, he puts the figure into such positions as tend to bring out and develope the ribs. In this way, the spine is necessarily acted on, while, in the Calisthenics, scarcely any effect is produced on it.

This opinion on the Calisthenics is founded on a careful examination of the effects of the postures, and of the exercises with the cane and triangle on the individual muscles of the chest and spine, while they were performed by several young ladies, (five in number,) who, being slightly twisted, had been taught how to do them.

Mr. Jenkins's plan of exercise is certainly more useful for girls who have a tendency to become twisted, and will probably prevent perfect figures from becoming crooked; but the attempt to remedy a distinct serpentine twist by such means is a mere loss of time.

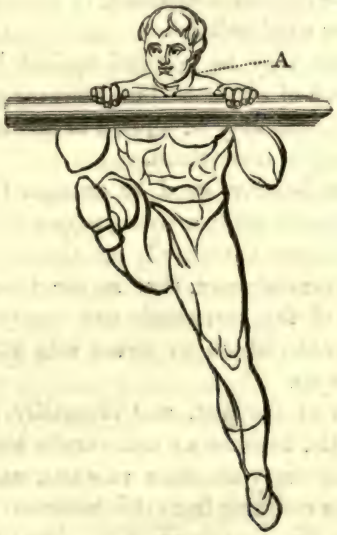
Is there any danger in performing those exercises which have of late years been recommended?

The bad effects of working a young horse too early, and so as to call for occasional violent exertion, are so generally known, that a valuable animal is seldom put to a trial of its powers before it has attained its full growth; but children, and especially those of the poor, are often put upon tasks far beyond their natural powers. The bad consequences are soon apparent, for children who are thus treated seldom grow up vigorously, but are stinted in their growth, and often have some bodily defect, or the elasticity and tone of their muscles are lost, long before the period at which they would have attained their full strength. Indeed, it is not uncommon for the poor to be worn out at an early age; and often may the face of twenty be seen on the shoulders of thirteen.

It may, perhaps, be said, that such children have not only been over-worked, but have been badly fed and clothed. There is no doubt that these causes will have the worst effects; but even nourishing food is not sufficient to avert the consequences

of premature over-exertion, especially if it be sudden, and entered upon without previous training*.

In considering this question, in relation to those who are not likely to be put to hard labour, we have particularly to fear that a child of high spirit, while climbing a rope, or mounting a cross-bar placed high above its head, may over-exert itself. The state of violent action that all the muscles, and particularly those of the abdomen, are thrown into, by attempting to raise the body on a cross-pole, or on the triangle, may be imagined by examining the sketch below, which is taken from one of the late works on gymnastics.



Within the last two years, several cases of rupture have occurred among those who have practised such feats; this accident has not been confined to young men, nor to boys, but one of the Directors of the exercises, a very powerful and muscular man, suffered in this way. But, although such serious accidents may not frequently happen, there are other circumstances which must not be forgotten in considering the propriety of

* The miserable appearance of some of the farmers' boys proves that even fresh air and plenty of good food will not counteract the bad effects of early and hard labour.

young persons practising gymnastics,—the changes which may be produced in the condition of the ligaments of the joints. When muscles are gradually increased in strength, the ligaments become strong in proportion; but the ligaments are as likely to be hurt from the muscles being suddenly called into violent action and at an early age, as by any accidental twist or strain. They are in this way liable to become spongy and relaxed, so as to produce weakness, or a condition similar to the joints of a young horse which has been galloped hard, or obliged to take great leaps before he has acquired his full strength; indeed, there is much resemblance in the condition of a joint with the ligaments strained, to that of a horse which is broken down or wind-galled*.

A violent sprain may be guarded against by gradually increasing the degree of exertion; but, however great the care in this respect, the joints may be injured in another way, which it is highly important to understand.

If any exercise, however good, be continued for a long time, and regularly repeated while a young person is growing, certain ligaments may become unnaturally lengthened and elastic. As, for example, we may observe, that in the bolero dance, upon the stage, some of the performers can nearly touch the floor with the inner ankle, which no person with a fine and strongly formed ankle can do.

The ligaments of the foot, and especially the lateral ligaments of the ankle, become so unnaturally long, that the foot can be turned in every direction as easily as the hand. The bad consequences resulting from this looseness of the joints, do not appear when the performer is dancing or strutting along the stage, as the muscles of the leg are then in an artificial state of exertion, and for a time preserve the bones in a proper relation to each other; but the effect is quite obvious when the dancers are walking in the streets, for then, while attempting to walk naturally, they have a shuffling gait. This is particularly the case in old dancers, who have retired from the stage; for the muscles having by disuse lost their tone, the bad effects

* Small bunyons or ganglions, which are similar to what the farrier calls wind-galls, are sometimes found about the ankle joints of delicate girls, who have over-exerted themselves in dancing.

of lengthening and straining the ligaments are then distinctly marked. Indeed these evils are not confined to a peculiarity of gait, for the feet of almost every opera-dancer, excepting those called the *pantomimes*, are deformed, and even some of the dancers, while in full vigour and most admired, are actually lame. This seems a bold assertion; but if a high instep be important to a well-formed foot, these dancers' feet are deformed, for, with few exceptions, they are quite flat; and that they are lame cannot be denied, as they have, almost all, a halt in their gait. If we consider the manner in which they are taught, we shall not be surprised at this. They commence their discipline at a very early age, and endeavour, for many hours daily, to stretch the ligaments of the feet and ankles. This is done in various ways, but chiefly by standing on the tips of their toes, their only respite being occasional attempts to push the ankle bones towards the floor. In this way the power of the muscles is soon exhausted, and the whole weight of the body being then sustained by the ligaments, they yield; and hence those which bind the bones of the foot together become unnaturally lengthened. As a necessary consequence of the stretching and elongation of the ligaments, the bones are separated from each other, and the feet are thus rendered nearly as flat as those of a monkey*. Although the shuffling gait and the lameness apparent in walking depend principally on the condition of the ligaments and of the muscles forming the calf of the leg, they

* The feet of those who wear strong shoes are frequently deformed, but in a different manner. The bones are pressed together, and the ligaments and small muscles, instead of being unduly developed, are nearly destroyed; this results from the shoes almost entirely preventing the natural action of the muscles. The effect of encasing the feet in the manner practised in China is generally known; there was an opportunity of proving, by examination of the feet of the Chinese woman, who died in Pall Mall, that the stucco models, brought from China, give a very correct idea of their form.

In connection with this subject, the curious difference between the foot and leg of an Irish haymaker and that of an English peasant may be noted. The effects produced by the heavy unyielding shoe, and the tight leather gaiters, which the Englishman wears, are very evident in the shape of his leg, for he has scarcely any calf, and when he runs, he drags his leg after him as if it were a lifeless mass attached to his thigh; while the Irishman, or a Scotch shepherd, being seldom encumbered with shoes, has strong and well-formed feet and legs, and, in running, bounds or springs from the toes.

may, in some degree, result from the dancer being so much accustomed to move on the tips of his toes, that it has become almost unnatural for him to bring his heel to the ground. Indeed the gait of an opera-dancer in walking may be said to resemble, in some respects, that of a bear dancing; for this animal, which, like all other quadrupeds, *walks* on the tips of his toes, when obliged to *dance*, must bring his heel, or os calcis, to the ground.

Some facts important to the question of the propriety of young persons performing gymnastics, may be learnt by observing the manner dancers prepare for exhibition on the stage. The many hours engaged in practice during the day is not sufficient, but they find it necessary to bring their muscles and ligaments into a certain condition, by gradually exerting and stretching them for an hour or more, immediately before appearing. All who have gone behind the scenes in the Opera must have observed the dancers in the green-room pass from simple postures and gentle exercises to those requiring the greatest exertion. By thus gradually suppling the joints, and exciting the muscles, the dancer is enabled, without danger of breaking a tendon, to enter with a bounding leap upon the stage, and to spring at once into full activity. But, notwithstanding all these preparations, dancers after a time appear lame; they seldom suffer from the violent sprain which is immediately attended with pain and swelling, but their ligaments become gradually over-extended. There is little danger of boys or girls falling into this state; but is it to be expected that thoughtless boys running out of school will submit to go through the necessary preparatory exercises, before entering on those more violent ones, which they consider more manly, and to be really *gymnastics?* If they do not submit to them, is there not danger of a tendon or the sheath of a muscle being ruptured, and thus render them uncomfortable for life*?

* Every surgeon knows that in young persons the epiphysis is not firmly united to the shaft of the bone, and that it is liable to be detached by the sudden wrench of a joint. This accident lately occurred to a young lady, at a school near town, while practising some of these exercises. The consequences were violent inflammation of the joint, and, notwithstanding the greatest care, death in three weeks.

If they escape this very serious misfortune, what is gained by a pre-eminence in performing violent exercises?—A species of deformity; at least, a form not to be admired. If young ladies are taught by a sensible dancing-master, and boys learn to fence, there will be little occasion for any formal exercises in addition to those taken in the various school games, and in skipping, playing battledore, &c.

As much has been said of the possibility of curing all distortions of the figure by particular exercises, it is necessary to make a few remarks on this question.

We have frequent opportunities of observing the good effects that are produced by a variety of exercises on young ladies who are in that state of listlessness and muscular inactivity which causes the chest to appear contracted, and even gives the appearance of curvature of the spine. The chest soon becomes expanded, and instead of continuing to walk in a stiff awkward manner, as if the spine was distorted, the girl carries herself erect and with an elasticity and pliancy of the whole figure. But what is, perhaps, of more importance, a complete change takes place in her constitution; as, for example, if she had an habitual cough, it will most probably have ceased*. Indeed, a change for the better may be observed in every organ, and particularly in the state of the skin, which being of the first importance in the animal economy, should always attract attention in such cases.

But although the most beneficial effects are produced by ex ercises, we shall be disappointed if we trust entirely to them in cases where the spine is actually distorted, and even when the curvature is slight.

Those who are ignorant of the variety in the diseases of the spine are apt to be deceived with regard to the effects produced

* The following curious remark is made by Fuller in his *Medicina Gymnastica*:—"And so I think it is no less obvious if exercise be as efficacious as I assert, that it is in our choice whether a cough shall run on to a consumption." The good effects which ensue from exercises would almost induce us to give entire credit to Fuller's views, and to treat a girl who has a cough nearly in the same manner as if her spine was distorted; for it has been frequently remarked by a mother, that her daughter has not had her winter-cough, since she began the system of treatment to remedy the curve of the spine.

by exercises. They may be led to believe that the distorted appearance of a girl proceeds from an alteration in the form of the bones, while it may be owing only to a degree of relaxation and weakness*. When they see a girl in such a condition get rapidly better, merely by going through certain exercises, they naturally suppose that the same means will be successful in every case. This opinion is strengthened by the circumstance, that even when the distortion depends on a change having taken place in the form of the vertebræ, this system of treatment improves the health and vigour of the patient so much, that she is soon able to carry herself better than before, and the deformity is consequently not so apparent. This induces the friends to persevere in the system, and it is natural for them to imagine that if so much benefit accrues from a short trial of exercise, a curvature, however bad, may be completely cured by perseverance.

This method of treatment has been considered a modern discovery; but the older authors were acquainted with it, and in their writings we find a variety of exercises described, as suitable to different diseases.

Portal, the French physician, in his work upon rickets, after

* It must have occurred to almost every practitioner to be asked why a girl appears crooked at one time and straight at another. The suspicion that the girl is twisted, is generally considered as the mere fancy of an anxious mother; but it is not altogether ideal, and the cause of the change in the form may, perhaps, be explained. Young persons are subject to a peculiar squint, which is generally removed by attention to the state of the bowels. Now, as the muscles for the support of the spine are as dependent on the bowels for the proper performance of their functions as those of the eye, why may they not occasionally be in a state to twist the body as those of the orbit are to twist the eye? In several instances where the parents had been led to believe that the spine was actually distorted, the patients were subjected to very severe modes of treatment. In these cases the practitioner was probably also deceived, and really imagined that the figure was restored by some particular and direct effect which exercises or confinement to the inclined plane had upon the spine, when in fact the removal of the curves depended almost solely on the improvement in the state of the bowels. But these remarks should not lead to the belief, that certain medicines which act on the bowels will be sufficient to remove the deformity. Appropriate exercises and rest have nearly as much effect on the bowels as the best medicines; but as both plans of treatment are good, the most rational mode of proceeding is to combine them.

some observations on the condition of the muscular power and shape of men who follow different trades, says, " Do not these afford so many lessons which should be remembered when we endeavour to restore the shape of children, whose figure or limbs have become distorted? We ought to consider the effects caused by using particular classes of muscles, as examples in favour of such exercises as will strengthen those muscles which are weakest."

In pursuance of these principles, he recommends a variety of exercises; some in the form of games, and such especially as are calculated to act upon certain muscles, as pulling weights over wheels fixed in particular situations, &c. But, although Portal has given us many useful hints, as he has taken erroneous views of the pathology of distortion, and has not made correct distinctions between the different cases, his advice, if followed in every instance, might not only be useless but even hurtful.

If all the distortions, to which the spine and bones of the chest are liable, depended on the same cause, and were in every respect exactly similar, the treatment found beneficial in one case might be reasonably expected to be good in all. But there is great variety in the cases of distortion; the whole figure may be in the relaxed condition alluded to, or the spine may be already twisted in consequence of a sponginess and lengthening of the ligaments, while the muscles are at the same time in a debilitated state. In either of these cases, appropriate exercises would quickly restore the figure; but in another instance, although the distortion might be apparently similar, the shape of the bones may be already altered. Exercises would still be beneficial; but unless a variety of means were also used to preserve the bones during the intervals of exercise in a favourable position, the vertebræ could not regain their natural forms, on the restoration of which the ultimate cure depends. It may be a question whether climbing ropes, &c., are the exercises the best calculated to remedy a curvature of the spine; but there can be no doubt that their immediate effect is to exhaust the vigour of the muscles by which the spine should be supported, and also to relax and lengthen

the ligaments by which the vertebræ are held together. Under these circumstances, little good can be done, if the patient be allowed to walk or ride, instead of being immediately placed in a proper position of rest. The apparatus which holds the different parts of the column together being thus relaxed and weakened, the spine yields to the superincumbent weight, and hence the individual vertebræ fall still farther out of their right line*. Patients should, therefore, never go through fatiguing exercises, unless they can take complete rest afterwards, and in such positions as would facilitate the growth of the bones and ligaments in the manner desired. But, in truth, the value of exercises has been over-rated:—notwithstanding all that has been said, both in this country and in France, *no cures* have been effected in this way; and the issue of many cases has been such as to prove, that exercises are chiefly useful in counteracting the debilitating effects of certain other necessary modes of treatment. When we hear of parents still hoping that their children will be completely cured merely by going through certain forms of exercise, we cannot help thinking it strange, that they should have overlooked the fact that many persons whose situation in life has doomed them to active and laborious exercise of the muscles of the limbs and spine, should continue to be as much or more deformed than weak and delicate girls. Even some of the watermen on the Thames continue distorted, although they are constantly engaged in rowing. But it has been said, that exercises can at least do no harm in cases of distortion: there is much danger in this opinion, for the curve may appear to depend merely on weak-

* There is much similarity between the curves of a tree and of the spine. In both, the first bend is at the lower part, and in each, the upper part *strives* to become perpendicular to the base by a series of curves. These curves of the tree will increase if some artificial support be not given, and so it is with those of the spine; we should therefore imitate the operations of the gardener so far as to prop up the spine, and particularly after the power of the ligaments and muscles which are the natural supports has been expended by exercises. But we should not carry this analogy too far; for were we constantly to support the curved spine, the muscles would be rendered useless. It would be an interesting experiment to try whether the wood of a tree, which had been some time encased in props, was equally dense and strong as one which had been left to support itself. It would probably be found in a state analogous to that of a bone which had not been used.

ness, when a very important change may have taken place in the condition of the bones, which a superficial observer may not discover. Some of the vertebræ may have become anchylosed or firmly united by bone. In such a case (which is not an uncommon one), violent exertions would be of no avail in restoring the figure, and would be actually dangerous. Exercises have been prescribed in cases even where the surgeon was aware that anchylosis had taken place, and they have been prescribed under the idea that where the spine is consolidated by bone it is stronger than in any other part, and is therefore in no danger of being broken even by the most violent exertion. But the opinion, that the spine is strongest where it is anchylosed, is founded in ignorance of its structure; for it has been proved, by various experiments, that the strength of the spine mainly depends on the elasticity of the peculiar matter by which the vertebræ are united.

To demonstrate that the spinal column is not strengthened but weakened by anchylosis, we have only to try the comparative degree of strength in the several parts of a spine where some of the vertebræ are united together by bone. In such an experiment we shall find that the anchylosed part is so weak and brittle, as to be fractured by the slightest jerk, while it is very difficult to break the column at any part where the intervertebral cartilages are entire. In further proof of this fact, there are several fatal instances of fracture recorded, where the spine was broken at an anchylosed part, by a comparatively slight degree of force.

In cases of lateral curvature where the patient has a bad constitution, it is not unusual for the bones to unite by anchylosis; but it is in another set of affections of the spine that we have chiefly to dread the misapplication of exercises. When the spine is attacked by scrophulous inflammation, which often destroys the bodies of two or more of the vertebræ, the condition of the patient at the commencement of the disease is such, that the slightest exertion produces great pain and an attack of fever; of course there can be no exercise used at this period. But if the patient should recover so far as to be able to walk, there is danger of particular exercises being recommended to cure the deformity consequent upon

this disease, because they have been found beneficial in other cases of distorted spine. It can scarcely be believed that any person acquainted with the changes which take place in the bones in such cases, would venture to recommend the trial of violent exertions to remove the curves; yet patients, whose spines had been distorted by disease of the vertebræ, have not only been obliged to mount ladders and climb ropes, but even to be suspended and swung by the head for half an hour, three times a day, under the direction of persons called regular practitioners. They who advised such means cannot be aware of the nature of the distortion that produces the *hump back*, for they would not intentionally allow a patient to run the risk of being killed by the slightest slip or accidental jerk*.

There is a set of cases very common in London, in which certain exercises may be detrimental instead of useful—when the distortion proceeds from rickets, or is accompanied by a ricketty affection of the limbs. Although the general character of the spine in such cases is very similar to that of the common lateral curvature, there is a very important difference between them; and unless exercises be carefully performed, and in a particular manner, the general distortion may be increased, and especially that of the bones of the pelvis, from which the most serious consequences may ensue.

There can be little doubt that confounding cases of distortion from rickets, with those which are independent of this specific disease of the osseous system, is the cause of the discrepancy of opinion which has prevailed on the propriety of exercises in cases of lateral curvature of the spine; for as long as no proper distinctions were made between the different causes of distortion, the question of exercise must have been perplexing. In some instances, certain exercises were evidently beneficial; while in others, apparently similar, they increased the distortion.

We think that we have now shown the necessity of every

* The following instance of the fatal issue of a case where the distortion was of that kind called hump-back, is given by a late author.—"On the 21st of October, she was lifted out of bed, and by some accident fell from the arms of the servant to the ground on her back; after screaming, she died in five minutes."

case being fully understood before violent exercises are proposed, and also that, although various exercises may be beneficial in almost every stage of lateral curvature when judiciously managed, they will be of little avail in restoring the figure, unless the bones be kept in proper relation to each other during the intervals of rest; nor should it be forgotten that the changes which take place in the form of the individual bones of the trunk are so gradual and so imperceptible, that an indiscriminate use of different kinds of exercise may be injurious even in the simplest case.

Under all these difficulties it may be affirmed, that unless the exercises are superintended by a person thoroughly acquainted, not only with the natural actions of the several classes of muscles, but also with the causes of certain changes in the form, they may be more detrimental than useful. When there is only a degree of lassitude, and before distortion has actually taken place, various exercises are safe and useful; but when the spine has become in the slightest degree distorted, it is necessary to pay strict attention to the effects produced by each kind of exercise, and even when employed merely as the means of keeping up the tone of the muscles while the patient is submitting to other plans of treatment.

On the Introduction of Salt as an Agricultural Agent.

In the few years which have elapsed since the late Sir Thomas Bernard first directed the general attention of the public to the immense value of salt for agricultural purposes, by the publication of his "Case of the Salt Duties," its progress has been almost great enough to satisfy even its warmest advocates; and although it has lost, in my valued correspondent, and in the late Samuel Parkes, Esq., two able friends, yet I rejoice to say that it will soon be in a state no longer to need the authority of a single name to induce its general employment.

The large and increasing sale of refuse salt to the farmers demonstrates the interest they have taken in its examination; and when we consider that a waggon can convey into the inland districts, where manure is generally the most valuable, salt

sufficient for four or five acres, at a charge of twenty shillings, we must allow that it is at least the cheapest manure in the possession of the farmer.

In the west of England, more especially, I find that the demand for agricultural salt has been very great; and, at the salt works of Droitwich, has often averaged sixty or seventy tons per week. The Bath and West of England Society have hence been induced to offer two prizes of 50*l.* and 25*l.*, for the most satisfactory instructions founded on experiments on not less than ten acres of ground, for its correct application.

It will not therefore, perhaps, be a subject unworthy of the attention of your readers, if I offer a few instructions as to the best modes of its application, founded on experiments, and on extensive correspondence with its advocates and opponents of several years' duration.

And for this purpose, it will be, perhaps, most satisfactory to the practical farmer, if I state what has been the result of the application of salt to two of the commonly-cultivated grasses, as wheat and barley; and I feel the more inclined to confine my observations to these two crops, because all my experiments on these have been confirmed by those of Mr. George Sinclair, of New Cross, which were conducted at Woburn for a series of years, with the greatest scientific precision.

All my experiments have shown that salt is best applied to wheat, when the seed is sown in a proportion of from ten to twenty bushels per acre: my own experiments have been conducted on a light gravelly soil in Essex, and have invariably demonstrated a large increased produce of wheat, from such application, varying from twelve to seventeen bushels per acre.

In the experiments of Mr. Sinclair, conducted on a much richer soil at Woburn, the good effects of salt manure were still more striking, and the increased produce much greater; but there his experiments being conducted on a very small scale, will account for the enormous produce he obtained, for in experiments on small plots the produce is generally very large.

In one of his valuable experiments, an application of only five bushels and a half of salt produced ninety-one bushels of wheat per acre: while, on the same soil, forty-five tons of spit manure produced but forty-five bushels of wheat.

In my Essay on the Uses of Salt, p. 86, second edition, I have given the valuable table of experiments conducted by this gentleman, and I cannot refer to a more copious series of facts.

The use of salt to prevent smut in wheat, by steeping the seed in its brine, is completely established; and some experiments which I have witnessed tend very strongly to confirm the assertion of the Cornish farmers, that salt entirely prevents the ravages of mildew.

According to Dr. Paris, salt has been applied to agricultural purposes for a very long period in Cornwall.

For barley, salt should not be applied in a larger proportion than sixteen bushels per acre, nor in less than ten bushels: on the light soils, in Great Totham, in Essex, I have always found that its application has been successful, invariably producing an increased produce of barley, of from eight to sixteen bushels per acre; and in some experiments in which I assisted in Suffolk with Mr. Ransone, of Sproughton, the increased produce was twenty-one bushels per acre.

In the experiments of Mr. Sinclair, a soil which produced twelve bushels of barley per acre, without any manure, produced, when eleven bushels of salt had been added before the seed was sown, twenty-eight bushels and a half of barley.

For turnips, mangel wurzel, and other green crops, salt has been very advantageously applied in the proportion of from five to fifteen bushels per acre; especially on the light soils of the west of England, a few days before the seed was sown. I find, by a communication with which I was lately favoured by Sir Thomas Ackland, Bart., that his bailiff has been very successful in its application.

And in the researches of Mr. Collyns, of Renton, in Devonshire, the author of "Ten Minutes Advice on the Use of Salt," the same good result was apparent: he states to me on the authority of some neighbouring farmers,—"I have found salt answer my most sanguine expectations for barley, oats, potatoes, and turnips, both as to the increased quantity and improved quality of the crops."

The use of salt as a condiment for cattle, sheep, and horses, has become very extensive since the repeal of the salt duties: the sale of the finest salt has more than doubled since their

removal; and the greater part of this increase has doubtlessly been for the purposes of live stock.

The avidity with which animals consume it is very curious: this is always evinced in a very decisive way when it is given to them in its pure state; and if a small portion of a meadow be well salted, the cattle will always eat it down closer than any other part of the same field.

This was strikingly shown on a salted portion of the park of Walter Long, Esq., in Hampshire: the deer, and other live stock, never left it while a blade of grass remained.

As the avidity with which animals consume salted hay is not so generally known as it ought, I will state the following fact:

Mr. Green, of Wargrave in Berkshire, having, in the season of 1824, a large quantity of coarse, sour, rushy hay, entirely worthless, it was stacked with a large quantity of salt. When the period arrived that his sheep required a supply of hay, he directed his shepherd to use the salted inferior hay, which the sheep consumed with the greatest avidity. The stack being finished, the shepherd was directed to supply them with the best hay he could find of other fine meadow hay. He came, however, the next morning to his master, and made the following remark:—"We, sir, must have made a great mistake, and forgotten which stack we salted; for our sheep will not eat the hay which we think the best."

For the general introduction of salt as a condiment for cattle, the farmer is mainly indebted to Mr. Curwen, the member for Cumberland, who has employed salt to his live stock, daily, for years.

For horses he gives	6 oz. per day.
„ Milch cows	4 „
„ Feeding oxen	6 „
„ Yearlings	3 „
„ Calves	1 „
„ Sheep	2 to 4 oz. per week.

Mr. Sully, of Wivelisscombe in Somersetshire, who has for some years given his horses two oz. of salt every day in their food, remarks, "The two oz. of salt will be found an excellent stimulus to the horse's stomach, and should, on no account, be omitted."

To this list I might add the experience of Mr. Wood, of

Ingatestone in Essex, and a host of others; but to the unprejudiced agriculturist one part will be sufficient.

The successful progress made in the introduction of salt, for live stock, will encourage its friends to persevere in the course they have so well begun; for even the use of salt for domestic animals, of which hardly a farmer now fails to avail himself, at least occasionally, even this question has had to encounter the most profound ignorance in all shapes; and the vengeance of the magistrates has been hurled against those who have used it to improve the condition of the horses entrusted to their care.

About sixty years since, some honest farmers' servants in Hampshire were brought before a magistrate at Winchester, charged by their ignorant master with the horrid crime of giving his cart-horses salt in their corn. "I should not have discovered it," said their master, "had not my horses grown *so fat*, and their coats become so sleek lately." "Salt for horses!" exclaimed the wise administrator of the law: "can any thing be more poisonous? The villains intended, no doubt, to make them full of humours and greese! Let them be committed to the Bridewell for a month!"

These men worked for a Mr. Earle of King's Worthy, near Winchester.

The employment of salt in horticulture has been on a very small scale; but the experiments hitherto made are decidedly in its favour.

I am indebted to my brother, Mr. George Johnson, for several important experiments in the kitchen-garden with salt: they were made with much care, and I can vouch for their correctness.

The soil was composed of

Stones and gravel principally siliceous	27 0	28 5
Vegetable fibres	1 5	
Soluble matters, principally vegetable extract		3 0
Carbonate of lime and magnesia		18 0
Oxide of iron		4 0
Animal and vegetable matter		1 0
Alumina		4 5
Silica		40 0
Loss		1 0
		100

The following is a statement of the produce of the crops grown on the soil.

Windsor Beans.

		Produce in Bushels per Acre.
Ex. 1.	Soil without any manure	135½
2.	Soil dressed with twenty bushels of salt per acre, a week before seed time	217

Onions.

		Tons.	cwt.	qrs.	lbs.
1.	Soil manured with twenty bushels of salt, and ten tons of farm yard manure	3	12	3	12
2.	Soil with twelve tons' yard manure	2	10	2	19

Carrots.

1.	Soil with twenty bushels of salt, and twenty tons' yard manure	23	6	1	18
2.	Soil with twenty tons' yard manure	22	18	0	26
3.	Soil with twenty bushels of salt	18	2	0	0
4.	Soil without any manure	13	4	0	0

Parsnips.

1.	Yard manure twenty tons, salt twenty bushels	6	15	0	0
2.	Yard manure twenty tons	6	11	0	0

Early Potatoes.

		Bushels.
1	Soil without any manure	308
2	Soil with twenty bushels of salt	584*

In a communication lately received from Mr. Hogg, an eminent florist of Paddington, he observes, " from the few experiments I have tried with salt as a garden manure, I am fully prepared to bear testimony to its usefulness: the idea that first suggested itself to my mind arose from contemplating the successful cultivation of hyacinths in Holland. This root, though not indigenous to the country, may be said to be completely naturalized in the neighbourhood of Haerlem, where it grows luxuriantly in a deep sandy alluvial soil; yet one great cause of its free growth I considered was owing to the saline atmosphere: this induced me to mix salt in the compost, and I am satisfied that no hyacinths will grow well at a distance from the sea without it. I am also of opinion that the numerous bulbous tribe of amaryllises, especially those from the Cape of Good Hope, ixias, alliums, (which include onions, garlic, shalots, &c.) anemonies, various species of the lily, antholyza, colchicum, crinum, cyclamens, narcissus, iris, gladiolus, ranunculus, scilla, and many others, should have either salt or sea-sand in the mould used for them."

* See Observations on Salt, fourth edition, p. 13.

" I invariably use salt as an ingredient in the compost for carnations; a plant which, like wheat, requires a substantial soil, and all the strength and heat of the summer to bring it to perfection."

That salt is alike beneficial to all soils, and at all times, is an assertion too absurd to need refutation: for such an universal property belongs to no other manure; even chalk or lime will not suit all soils. Farmyard manure may be used without advantage. When chalk is applied to some soil, years must elapse before its good effects are visible to the farmer; " and yet," said the late Lord Erskine, " chalk, which has caused to start into life the most inert soils, is just nothing as a manure, compared with salt."

I am quite willing to admit that salt has been sometimes employed with detriment to the crop under experiment, and often without effect; but these facts must not be taken as decisive proofs of its inutility, even on those soils: for some soils require it to be applied in the autumn, others in the spring; some crops are most benefited by having it applied to the soil long previous to their insertion, others at that immediate period. Neither let any one imagine, because it is beneficial to light soils, that it cannot be equally so to more tenacious ones: its moistening powers render the first more fertile, the latter more friable, and open to every agricultural operation in the driest seasons.

It is scarcely necessary to add, that no one ought to view the subject as of small importance. Saying nothing of it in a scientific point of view, that cannot be unimportant with which is connected one of the staple manufactures of the country; which concerns the cheapest and most portable manure in the possession of the agriculturist. Let no one think he can do nothing in furtherance of the research; the most ill-directed and unsuccessful experiment serves, at least, as a warning, a beacon to others—a judicious one especially, when crowned with success, bears with it the inestimable gratification, that a benefit is conferred upon mankind, and the pleasure that always accompanies the illustration of truth.

C. W. Johnson.

Great Totham, Essex, June 1, 1827.

Description of the Kaleidophone, or Phonic Kaleidoscope; a new Philosophical Toy, for the Illustration of several Interesting and Amusing Acoustical and Optical Phenomena.

[Communicated by Mr. C. Wheatstone.]

The application of the principles of science to ornamental and amusing purposes contributes, in a great degree, to render them extensively popular; for the exhibition of striking experiments induces the observer to investigate their causes with additional interest, and enables him more permanently to remember their effects. I shall not, therefore, need an apology for presenting the tyro in science with another combination of philosophy with amusement, in addition to those already extant.

But this instrument possesses higher claims to attention; for it exemplifies an interesting series of natural phenomena, and renders obvious to the common observer what has hitherto been confined to the calculations of the mathematician; it presents another proof, that however remote from common observation the operations of nature may be, the most beautiful order and symmetry prevail through all.

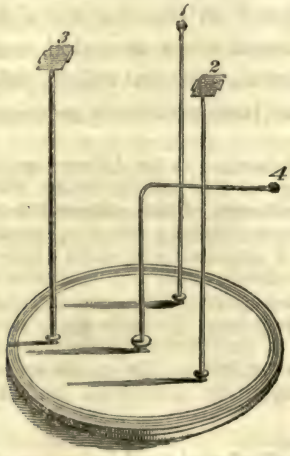
In the property of "creating beautiful forms," the Kaleidophone resembles the celebrated invention of Dr. Brewster, from which its name is modified; but to the instrument itself, and its mode of action, it is almost superfluous to say there is no similarity. Previously to entering into an explanation of its construction and effects, the following brief summary may suffice to give a general idea of the nature of the experiments it is intended to illustrate.

These experiments principally consist in subjecting to ocular demonstration the orbits or paths described by the points of greatest excursion in vibrating rods, which in the most frequent cases, those of the combinations of different modes of vibration, assume the most diversified and elegant curvilinear forms *. The entire track of each orbit is rendered simulta-

* We are indebted to Dr. T. Young for the first observation of these phenomena; the following account of his experiments is quoted from the *Philosophical Transactions* for 1800. "Take one of the lowest

neously visible by causing it to be delineated by a brilliantly luminous point, and the figure being completed in less time than the duration of the visual impression, the whole orbit appears as a continuous line of light. As besides the changes which result from the combinations of the primitive with the higher modes of vibration, the figures of the orbits are affected by the form of the rod, by the extent of the excursions of the vibrations, by the mode of producing the motions, and by many other circumstances, a great variety of pleasing and regular forms is obtained. This variety is also enhanced by giving the same motions to a number of symmetrically disposed luminous points, the mutual intersections of the orbits of which produce innumerable elegant forms; and the appearances may be still more variegated by occasionally causing these points to reflect differently-coloured lights.

The apparatus for exhibiting these experiments consists of a circular board about nine inches in diameter, into which are



perpendicularly fixed, at equal distances from the circumference and from each other, three steel rods each about a foot in length.

strings of a square piano-forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very ac-

The first rod is cylindrical, about 1-10th of an inch in diameter, and is surmounted by a spherical bead * which concentrates and reflects the light which falls upon it. The second is a similar rod, upon the upper extremity of which is placed a plate moving on a joint, so that its plane may be rendered either horizontal, oblique, or perpendicular; this plate is adapted to the reception of the objects, which consist of beads differently coloured and arranged on pieces of black card in symmetrical forms. The third is a four-sided prismatic rod, and a similar plate is attached to its extremity for the reception of the same objects. Another rod is fixed at the centre of the board; this is bent to a right angle, and is furnished with a bead similarly to the first-mentioned rod. A small nut and screw is fixed to the board near the lower end of the first rod, in order by pressing upon it to render occasionally its rigidity unequal. A hammer, softened by a leather covering, is employed to strike the rods; and a violin-bow is necessary to produce some varieties of effect.

I shall now proceed to describe the different appearances which the rods present when in action, and to give directions for the production of the different effects, following the order in which the rods have been previously mentioned.

curately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing than the multifarious forms of the quiescent lines of vibrating plates discovered by Professor Chladni; and it is, indeed, in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it."

The extremely limited extent of the excursions of a vibrating chord prevents its motions from being distinctly observed by the naked eye, but as the rods employed in the present experiments can extend their excursions to nearly two inches, and as the means employed greatly increase the intensity of the light, the phenomena are exhibited in a far more evident manner.

* The only beads well adapted for this purpose are made of extremely thin glass silvered on the interior surface, and about one-sixth of an inch in diameter; they are to be obtained at the shops under the name of steel beads. The protuberances at the apertures must be removed or blackened, otherwise the reflections from them will render the images confused. To produce the coloured tracks, these beads must be coated with transparent colours, such as are ordinarily used for painting on glass; the light will then be reflected through the coloured surface; but in beads made of coloured glass, the reflection being made from the external surface, shows only white light. The bead is cemented into a small brass cup screwed to the top of the wire.

No. 1. On causing the entire rod to vibrate, so that its lowest sound be produced *, as it is seldom that the motions of a cylindrical rod can be confined to a plane, the vibrations will almost always be combined with a circular motion. When the pressure on the fixed end is exerted on two opposite points, and the rod put in motion in the direction of the pressure, the following progression in the changes of form will be distinctly observed: the track will commence as a line, and almost immediately open into an ellipse, the lesser axis of which will gradually extend as the larger axis diminishes, until it becomes a circle; what was before the lesser will then become the larger axis, and thus the motions will alternate until, from their

* The most simple mode of vibration of a rod vibrating transversely, when one of its extremities is fixed and the other is free, is that in which the entire rod makes its vibrations alternately on each side of the axis, which is nowhere intersected by the curve, but only touched at the fixed end. This gives the gravest sound which can be produced from the rod. In the other modes of vibration, the axis is intersected by the curve 1, 2, 3, or more times. The best means to command the production of these sounds is to touch a node of vibration lightly with the finger, and to put a vibrating part in motion by a violin-bow. In the second sound, the number of vibrations is to that of the first, as $5^2 : 2^2$, or as 25 : 4; the difference of the sounds is, therefore, two octaves and an augmented fifth. Separating the first sound from the series, the number of the vibrations of all the others will be to one another as the squares of the numbers 3, 5, 7, 9, &c.; the third, in which there are three nodes, will therefore exceed the second by an octave and an augmented fourth; in the fourth, the acuteness will be augmented by nearly an octave; in the fifth, by nearly a major sixth, &c. To reduce to the same pitch all the proportions of the sounds which such a rod is capable of producing, I shall regard the sound corresponding with the most simple motion as the C one octave lower than the lowest of the piano-forte; the proportions of the sounds will then be—

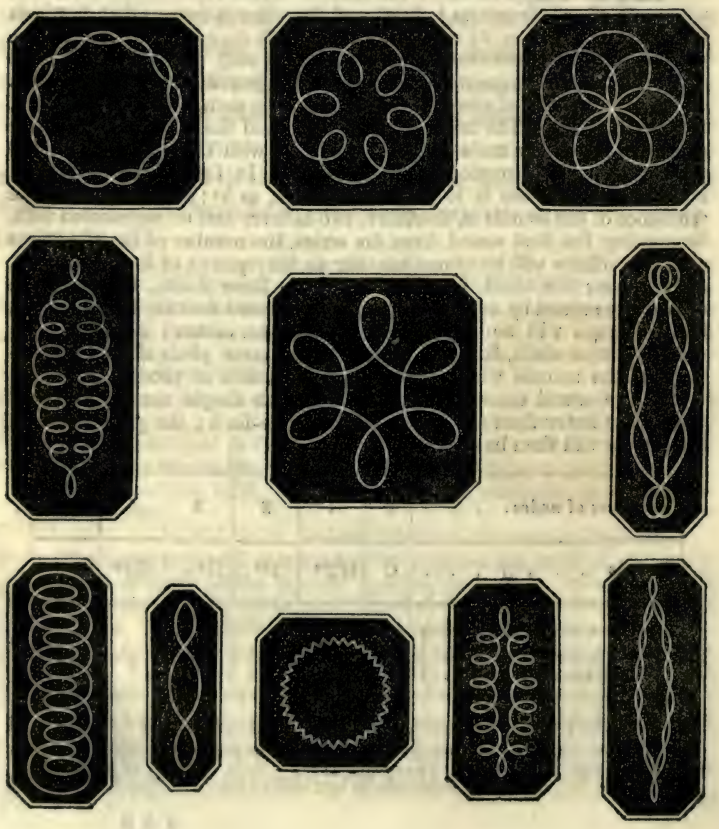
Numbers of nodes	0	1	2	3	4	5
Sounds	$\overline{C}$	$G\sharp^2$	D^4	D^5-	$B\flat^5$	F^6+
Numbers, the squares of which correspond with these sounds	(2)	(5) 3	5	7	9	11

The possible series of sounds, regarding the fundamental as unity, will therefore be—1, $6\frac{1}{4}$, $17\frac{1}{36}$, $34\frac{1}{36}$, $56\frac{1}{4}$, &c.; or expressed in integral numbers—36, 225, 625, 1225, 2025, &c.—Chladni, *Traité d'Acoustique*, p. 91.

decreasing magnitudes, they cease to be visible. In the case just described the ellipses make a right angle with each other; but by altering the direction of the motion, so as to render it oblique to the direction of the pressure, they may be made to intersect under any required angle, and when this angle $= 0$ the motion will be merely vibratory.

Every single sound formed by the subdivisions of the rod will present similar appearances, but the excursions will be smaller as the sound is higher, or, which is the same thing, as the number of the vibrations increases.

In the most simple case of the co-existence of two sounds, shown by putting the entire rod in motion, and producing also a higher sound by the friction of a bow; the original figure will

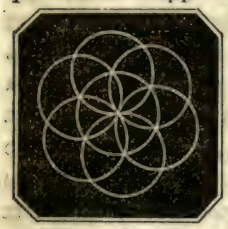


appear waved or indented, and as unity is to the number of indentations, so will the number of vibrations in the lower sound be to the number in the higher sound. On varying the mode of excitation, by striking the rod in different parts and with different forces, very complicated and beautiful curvilinear forms may be obtained: some of these are represented by the opposite figures.

Placing the hand on the lower part of the rod, below the place at which it is excited, the excursions of the motions will rapidly decrease and exhibit spiral figures.

To obtain the figures with brilliancy and distinctness, a single light only should be employed, as that of the sun, a lamp, or a candle; rays of light proceeding from several points, as from a number of candles, or from the reflection of the clouds, occasion the track to be broad and indistinct; but double lights may be employed with effect, provided they be of equal intensity and symmetrically placed; each bead will then describe two similar figures. The appearances, in a bright sunshine, are remarkably vivid and brilliant.

No. 2. Although very beautiful and varied forms may be produced from the motion of a single point, yet the compound figures which are presented by objects, formed by a number of points, offer appearances still more pleasing to the eye.



An object being placed horizontally on the plate, and the rod being put in motion, the mutual intersections of the points, each describing a similar figure, present to the eye complicated, yet symmetrical figures, resembling elegant specimens of engine-turning.

When the plate is horizontal, the figures are all in one plane, but if it be inclined or perpendicular, the curves being then made in parallel planes, gives the idea of a solid figure, and in some cases the appearances are particularly striking.

Complementary colours alone should be employed in the objects; for these harmonising together, give greater pleasure to the eye than an injudicious combination of discordant tints: the intensities should be occasionally varied, and colourless light intermingled with the different shades.

No. 3. When this prismatic rod is put in motion, in the direction of either of its sides, the points move only rectilineally, but when the motion is applied in an oblique direction, a variety of compound curves is shown: this rod is principally employed to exhibit the optical phenomena, which will be afterwards mentioned.

No. 4. When a rod is straight, the curve produced by any point describing its motion, is always in the same plane; but in a rod bent to any angle, the two parts moving most frequently in different directions, curves are produced whose parts do not lie in the same plane. A few trials will soon indicate the best way of applying the motion, so as to cause the two parts to vibrate in different directions.

Before the conclusion of this subject, I will avail myself of the opportunity of observing, that the application of this mode of experimenting may be extended to the delineation of every description of curvilinear and angular motion, when the amplitudes of the tracks are not too great; and by this means, it is not improbable that the experimental knowledge of many interesting principles in science may be facilitated.

On the Duplication and Multiplication of Objects.

When dark objects are substituted for luminous ones, their tracks become nearly invisible, and from the longer duration of the visual impression at the limits of vibration, the images are multiplied in proportion to the number of points at which they are retarded. Place horizontally on the rod No. 2, a word printed or written on a piece of card; in the lowest mode of vibration, at the opposite limits of the excursions, two legible images of the word will be distinctly seen, and but an indistinct shade, occasioned by the tracks of the letters, will appear in the intermediate space: the vibratory motion is imperceptible to the eye; the images will, therefore, appear stationary in this respect, but the diminution of the excursions will cause them to approximate very slowly and gradually towards the centre: this diminution operates so gradually, as to allow the images to superpose each other completely at each recurring vibration, without producing any intermingling or confusion.

On placing the object perpendicularly, the two images will

appear in parallel planes, the furthest image appearing through the first apparent surface. Small pictures have a singular effect applied in this manner.

When other sounds co-exist with the fundamental, the images are multiplied, but they become fainter as their number increases: these multiplied images are equally visible, whether the vibrations be rectilineal, elliptical, or circular.

A New Optical Experiment.

As that property of vision which occasions the apparent duration in the same places of visible images, after the objects which excite them have changed their positions,* has enabled us to submit to inspection the phenomena above described, it may not be irrelevant to subjoin a description of an apparatus which illustrates the transient duration of the impressions of light in a very evident manner.

At the back of a wooden frame, about six inches in height and breadth, and from one to three inches or more in depth, a circular plate of glass is placed, upon which a design is painted with transparent colours; at the front, is placed, parallel to the glass, a circle of tin, covered on its exterior surface with white paper, and having the space between two adjacent radii cut out. This circle moves freely on its centre round an axis, supported by a bar in front, and is put into rapid and regular motion by the application of any mechanical principle proper for the purpose; and a catch is so placed, that when the motion ceases, the aperture shall be concealed by the bar which supports the axis.

If a light be placed behind the transparent painting, and still better if it be concentrated by a lens, on making the circle revolve with rapidity, the whole of the picture will be rendered visible at one view, although but very limited portions are successively presented to the eye.

The intensity will differ in proportion to the excess of the transmitted light above that which falls in front of the circle; it will, therefore, increase the distinctness of the picture, to darken the latter as much as possible.

* It has been proved by the Chevalier d'Arcy, from the only experiments approaching to accuracy which have been made on this subject, that the extent of this duration is eight thirds. See his "Memoire sur la durée de la sensation de la vue:" Hist. de l'Acad. An. 1765.

London in 1826, *with respect to its present State of Architectural Advancement. Read before the Literary and Philosophical Society of Liverpool: Sessions* 1826.

Having lately had opportunities of examining the external aspect of the British Metropolis with much attention and in great detail, and having compared it with such of the other European capitals as I have seen, and with the rest, from a careful inspection of plans, descriptions and drawings, it has occurred to me that a few observations on the subject would not be altogether undeserving the attention of this society.

I find it generally admitted that, with respect to architectural improvement, London has made greater advances since the late peace, than in the entire century which preceded that most auspicious event. Public spirit has been more directed into this channel; the remission of war has enabled the nation to devote more expense to such objects; and above all, a taste for architectural embellishment, both in public buildings and private dwellings, has accompanied the natural progress of arts and knowledge in this refined community. If the career which has so lately been entered upon should happily not be interrupted by the recurrence of war, a still greater improvement in this most striking and resplendent of all the arts may be confidently anticipated.

As the architectural pretensions of London are therefore, in a very great degree, of a recent date, the subject is not destitute of novelty; no writer, as far as my knowledge extends, having as yet published any notices of the British capital in its present state. The inquiry must be admitted to be interesting, since the vast city, in its actual condition, presents a spectacle to which the history of ancient and modern civilization, in all probability, affords no parallel. Within the limits of this civilization, there is reason to believe so large a mass of human beings have never before been congregated on so small a space of ground. As to the great cities of antiquity, our information concerning their size and population is too vague, as well as too scanty, to enable us to speak with any precision If we are to believe the exaggerations of Herodotus and other

ancient writers, the cities of Nineveh and Babylon were of a magnitude to which nothing in subsequent ages will bear a comparison; but even if their extent be admitted as described, the manner in which they were built and inhabited would still afford grounds for believing that their population did not equal that of London. The pretensions of ancient Rome are somewhat more plausible. The development, however, of the ruins of Herculaneum and Pompeii have afforded us an insight into the comparative density of the inhabitants in ancient and modern towns, which tends much to abate our estimate of the eternal city; so that, rejecting the extravagant notions of Vossius, Lipsius, and other modern writers, we may, after a careful comparison of circumstances, suppose with Hume, that the population of ancient Rome may have reached somewhere about a million. The other great cities of ancient times, such as Ephesus, Agrigentum, Syracuse, Antioch, Alexandria, &c., need not be mentioned, since no calculation that I have seen raises their population to a million. With respect to modern ages, it has been thought by some writers that two or three cities of China and Japan exceed the limits of the British capital in size and population; but in the first place, no accurate census of these cities has ever been taken, and in the second, it is believed that a considerable portion of their inhabitants consists of hordes of Tartars or other migratory tribes; so that these places may safely be put out of the question in any inquiry which respects the cities of the civilized world. Our information concerning their detail is besides too imperfect to be made the basis of any observations.

This digression concerning the unexampled magnitude of our metropolis is not wholly impertinent in estimating its physical properties as a city. Many of its peculiarities, and some of its principal defects, arise necessarily from its enormous extent.

London owes its pre-eminent size to a conjunction of circumstances which have never concurred, in any other instance, to the formation of a great capital:—1. It is the greatest seaport in the world; 2. It is the greatest manufacturing city ever known; 3. It is the seat of the most opulent government that ever existed. The first of these assertions admits of no

dispute. The second seems somewhat more doubtful, because many other places appear to exhibit a greater mass of manufacturing industry; but in this point we are deceived by the vast extent of the metropolis, and the consequent diffusion of its scattered manufacturers. According to a late statement of Mr. Huskisson, the silk manufacture alone now reaches the enormous amount of fourteen millions sterling per annum, and is consequently, after cotton, the greatest staple of the country. Of this immense fabric, London is the principal seat, and it nearly engrosses many others of a smaller extent, but yet forming an immense aggregate when taken collectively, such as jewellery, plate, porter, books, newspapers, and many others.

Being, then, unquestionably the richest, largest, and most populous city of Europe; the seat of a wealthier court, and a more opulent body of nobility and gentry, than any other metropolis; it seems only a natural and reasonable expectation that it should likewise excel all others in the number and magnificence of its public edifices and private dwellings; in its general elegancies and accommodations as a city, and in all the appendages and decorations which naturally accompany so manifest a superiority. But this is very far from being the case, and our striking deficiencies in these points afford matter for one of the severest of our national reproaches. An overgrown capital is essentially a great evil, as it is the natural confluence of the worst species of misery, corruption, and crime. It brings together the greatest extremes of luxury and poverty, of rudeness and refinement. The natural, though insufficient compensation for these excesses, is sought for in the splendour of its decorations, and the superiority to which the ornamental arts of life may there be expected to attain. The art of architecture especially, the only one of the arts which requires great wealth for its successful prosecution, may there be expected to rear its head in proud pre-eminence; but the enlightened traveller, who comes to London with this very justifiable prepossession, will find himself miserably disappointed. Till within the last few years, that most splendid and impressive of all the arts has been almost wholly neglected, and the extension and improvement of the capital was left in the hands of sordid ignorance and barbarous caprice.

So much for the general view of the subject: I shall now beg leave to descend to a few more minute particulars.

The great architectural superiority of London, such as it is, consists in the number, size, and general neatness of its principal streets and squares. No other city can boast so great a number of streets, equal to those of London in length and breadth, and so uniformly well built. Petersburgh, Berlin, Naples, Turin, Genoa, Antwerp, Edinburgh, and other places, have perhaps finer streets than any in London, but in respect to their number there is no comparison. At present, also, it may be affirmed, that since the completion of the line of street extending from Carlton-house to the Regent's-park, it would not be easy to find in any other city, even a single street which greatly exceeds that magnificent range of buildings.

Remarks of a similar kind may be applied to the squares. If we assent to the critical canons established on this point, that architectural beauty is constituted by variety amidst uniformity, and that in every regular figure the subordinate parts should bear a certain proportion to each other, it will be found that every square in London is manifestly imperfect. There is none that can be compared with the great square of St. Petersburgh, the *Place Vendôme* at Paris, the *Place Royale* at Brussels, the *Place* of *St. Marc* at Venice, or even with the principal squares in Edinburgh. Yet, when we consider their number, their spaciousness, their enclosures, and their general neatness, they form altogether such an assemblage as no other city can rival. The new squares, such as Bryanstone, Torrington, Bedford, Russell, &c., are tolerably uniform, but they want variety and architectural decoration in their constituent parts. The old, such as Grosvenor, Portman, St. James's, &c., have sufficient variety in parts; but having neither uniformity nor proportion in their whole, nor much beauty in their details, their pretensions are still less. Fitzroy, Tavistock, Belgrave, and Regent-squares, if completed on their respective plans, as far as they are at present developed, will comprise most of the requisites of a handsome square, though not without considerable faults.

If we descend from these general features, to the individual mansions which form this stately assemblage of streets and squares, we shall find them, for the most part, utterly beneath

all criticism. An educated foreigner is quite astonished when shown the residences of our higher nobility and gentry in the British capital. Knowing them to be the wealthiest body in Europe, and knowing also that England is a country which pretends to superior refinement and civilization, he naturally expects to see the city filled with such palaces as the Pitti, Borghese, Colonna, Barberini, and the other celebrated mansions of Genoa, Florence, Naples, and Rome; or at least such as the principal *hotels*, or town residences of Paris, Petersburgh, or Turin. He has heard speak of some great nobleman, with a revenue equal to that of a principality. He feels a curiosity to look at his *palace*, and he is shown a plain, common brick house of forty or fifty feet in extent! Is it wonderful that he turns with contempt from such tasteless poverty of spirit, and feels inclined, like Buonaparte, to pronounce us a nation of shopkeepers?

In the capital article of churches, London will probably be admitted, after Rome, to take the first rank amongst the cities of Europe. Some few places, as Milan, Strasburgh, York, &c., may boast a finer Gothic cathedral than Westminster; and St. Paul's must yield the palm to St. Peter's as a Grecian temple; but no single city can boast *two* several specimens which rank so high in their respective departments. In churches of the secondary order London is also richer than any other city. Many of these are undoubtedly very handsome; but the design for which they are built, that of containing as large a congregation as possible, has created an uniformity of structure very wearisome to the eye. They consist mostly of a plain, oblong building, with a tall and slender steeple at one end, surmounted by a spire, and supported by a bold and handsome Corinthian or Ionic portico or peristyle; but the remainder of the church being generally destitute of all architectural ornament, a striking appearance of incongruity obtrudes itself on the spectator. This is greatly relieved when, as in the fine church of St. Martin's-in-the-fields, the body of the church is sustained by semi-columns or pilasters, and their proper entablature; but this is seldom attempted. The new church in Langham-place has a beautiful portico in the form of a hexastyle; and, though that structure is liable to much censure, yet the attempt at variety

which appears throughout the whole design is spirited and laudable.

In respect to palaces, which is also an important department in the architecture of a great city, London is confessedly excelled by almost every other capital in Europe, both in public and private edifices of this description; of the former, Whitehall, Carlton-house, and the Mansion-house, comprize the whole list of buildings any way entitled to the appellation of palaces, and even *their* title has often been thought disputable. The first of these three, considering the smallness of its dimensions, is eminently chaste and beautiful; and the two latter, though they have been much ridiculed by travellers, are not destitute of grandeur. In respect to the private mansions of our *grandees*, the deficiency, as noticed already, is still more deplorable. Here, indeed, is the great and glaring *opprobrium* of London, considered as the wealthiest city in Europe. There are scarcely half a dozen houses that would strike a stranger as fit for the residence of a British nobleman of 100,000 or 150,000*l.* a year. Of these, the principal are Burlington-house, Piccadilly; Lansdown-house, in Berkeley-square; Northumberland-house, Charing-cross; and Earl Spencer's, in the Green-park; and even of these four, the two first are shut up by hideous brick walls from the public eye. It might be expected, as a matter of course, that the British capital should contain at least five hundred houses of this description. In defence, however, of the British aristocracy, it may be urged, that it is more consonant to the national taste, and more conducive to the general decoration of the country, that they should reserve their superfluous wealth, and their architectural pride, for their family residences in the country. This feeling is, no doubt, highly commendable, and it has covered the land with a profusion of rural palaces such as no other nation can boast, and which give to many districts of the island the air of a continuous park or ornamented country. In addition to this, it may be alleged, that the limits of the metropolis itself are hardly capable of admitting any very great congregation of capital mansions. The immense size of London is already attended with great inconveniences and serious evils; but this extent would be enormously increased, if rows of palaces like those

of Genoa and Berlin were included in its circumference. As almost all the higher nobility and gentry have a house in the capital, and as the number of our opulent families is far beyond that of any country that ever existed, it seems necessary that the bulk of them should be satisfied with ordinary dwellings, such as those which constitute the principal streets of the west. It is this necessity which in some degree justifies, though it did not originally produce, the comparative meanness of the great family residences of London.

But after allowing the utmost weight to these considerations, still enough remains in the general aspect of London to excite surprise and regret in the mind of every one who feels any anxiety for the honour of the country, or the progress of the liberal arts. The prime nobility of the land, at least, might be expected to feel the generous ambition of distinguishing their families, and of decorating the metropolis of their country by town-residences worthy of their name and wealth, Many of their present dwellings occupy as much space, and have required nearly as much expense, as though they had been models of the finest Grecian architecture, instead of the tasteless edifices that now fill the interminable lines of the west of London with tiresome uniformity. Can any thing, for example, be more disgraceful to the noble house of Cavendish and its princely revenues, than the factory-looking building called Devonshire-house, which fills a space large enough for a Genoese or Roman palace? and this disgrace is rendered more apparent by its juxta-position to that fine mansion Burlington-house, almost the only town-residence which is really fit for a British nobleman. The same remark will apply to Norfolk-house, Chesterfield-house, the Marquis of Stafford's, Marlborough-house, and many others. Nor is it a little surprising that none of our great commercial men are seized with the architectural ardour which, in the middle ages, inspired the merchants of Venice, Genoa, and Florence, and urged them to ornament their respective cities in a way which has proved a perennial source of celebrity and wealth. It is not less singular that the British capital should not catch the spirit of emulation from the sister-capital of Scotland, which contains but few nobility or opulent gentry, and scarcely any

great merchants or manufactures, and which, nevertheless, in the opinion of many travellers, is actually the finest city in modern Europe.

We come next to the article of bridges; and in this particular, it must be admitted that no city in Europe can pretend to any competition with London. No other great capital is indeed so favourably situated in this respect, bisected as it is by a broad and deep river which bears on its current a greater commerce than any yet known in the annals of mankind. The Seine at Paris is very complete in its bridges, both as to quality and number; but the narrowness of the river is a bar to all magnificence. The bridge over the Garonne at Bordeaux is, perhaps, of greater extent, and that over the Elbe at Dresden is more finely ornamented than any in London, but no single city can show five such bridges as those of Westminster, Waterloo, Blackfriars, Southwark and London.

In its general public buildings, under which may be included every public edifice not comprehended under the heads of churches, palaces, or bridges, London, considering its vast extent, is certainly not pre-eminent, though it is by no means so strikingly deficient as some have supposed. It is a great disadvantage to the English metropolis, that its principal edifices are scattered over such a vast extent of surface, that they present an appearance of paucity much beyond the reality. The Custom-house, the New Post-office, the Mint, the Royal Exchange, the Bank, Somerset-house, the New Treasury, the Opera-house, and many others that need not be enumerated, would form a magnificent assemblage, if they were grouped nearly together, and could be viewed with little intermission of space and time. The two national hospitals of Greenwich and Chelsea may also be considered as belonging to London, and they form a noble appendage to its catalogue of public edifices. Greenwich hospital may, perhaps, be pronounced, without exception, the finest *profane* building (using that term in contradistinction to *sacred*) in modern Europe; and that of Chelsea, though deformed by the barbarous mixture of stone and brick which was so much in fashion in the earlier part of the seventeenth century, has an air of sedate grandeur which is very impressive.

The architectural spirit which has arisen in London since the late peace, and ramified from thence to every city and town of the empire, will present an era in our domestic history. The new erections in Regent-street and its appendages, in Pall-Mall, the Hay-market, and, above all, in the Regent's Park, discover the dawning of a new and better taste, which, though very far from the perfection to which we have a right to aspire, is yet, in comparison with that which preceded it, a just subject of national exultation. Regent's Park, and its circumjacent buildings, promise, in a few years, to afford something like an equipoise to the boasted *Palace-groupe* of Paris. If the plan already acted upon is steadily pursued, it will present an union of rural and architectural beauty on a scale of greater magnificence than can be found in any other place. The variety is here in the detached groups, and not as formerly in the individual dwellings, by which all unity and grandeur of effect was, of course, annihilated. These groups, undoubtedly, will not always bear the eye of a severe critic, but altogether they exhibit, perhaps, as much beauty as can easily be introduced into a collection of dwelling-houses of moderate size. Great care has been taken to give something of a classical air to every composition; and with this object, the deformity of *door-cases* has been in most cases excluded, and the entrances made from behind. The Doric and Ionic orders have been chiefly employed; but the Corinthian, and even the Tuscan, are occasionally introduced. One of these groups is finished with domes; but this is an attempt at magnificence which, on so small a scale, is not deserving of imitation.

The four parks of London ought not be omitted in any enumeration of its superior advantages. They form an unique and splendid appendage to the metropolis, such as no other city can boast; and, though not equal to the gardens of the Luxembourg and the Tuileries in minute beauty, and in convenience of access, are far beyond them in all the great requisites of a place of public recreation. The four new gateways to Hyde-Park, just completed, will form a beautiful improvement. Those who wish to be convinced of the fine effect which Doric architecture is capable of producing, even on a small scale, have only to inspect these edifices, where the finest

forms of elegance and grandeur are exhibited in a size of structure that in vulgar hands would have been considered as insignificant.

Such is my estimate, according to the best judgment I have been able to form, of the architectural pretensions of the British metropolis. In extenuation of its innumerable defects and deformities, it may be urged that London, as well as most of the old capitals of Europe, having arisen from small beginnings, and mostly in barbarous times, were built as chance or caprice directed, without taste or plan, and without any ambition of architectural distinction: hence the greatest part of each of them is old, irregular, mean and confined; abounding in narrow streets, crooked lanes, and ungraceful appendages. Such is especially the case with London, Paris, Vienna, Naples, Amsterdam, Madrid, Lisbon, Milan, Moscow, and several others. Most of them, also, having been designed for fortified places, it became necessary to be sparing of ground, and to crowd the inhabitants into the smallest possible space. St. Petersburgh is the only one of the capitals of Europe which has been built in modern ages and on an integral and preconcerted plan. It had likewise the singular good fortune of being erected under the auspices of an arbitrary and enlightened monarch, in whose view private inconveniences were as nothing, and general effect was the sole consideration. These felicitous circumstances have not produced all the advantages which might have been expected, but still the city of St. Petersburgh, as a whole, is very superior to any of the rest. Similar advantages have been enjoyed, in a smaller degree, by a few others of the European capitals, as Turin, Carlsruhe, Stockholm, Copenhagen, Berlin, Dublin, and Edinburgh; but these are only cities of the second and third rate. It is much to be lamented that the fire of London did not happen under the reign of George IV. Under the influence of modern taste the whole city would have risen from her ashes with a splendour and magnitude which would have astonished the world, and attracted the curious traveller from the remotest regions. She is now too old and unwieldy to expect any effectual cure of her complicated maladies, but her constitution is in a source of gradual improvement. The passion for archi-

tectural decoration has happily acquired all the dominant power of fashion, so that no extensive plan of building is now undertaken, in any principal part of the town, without some regard to external appearance. If this sudden fashion should happily not pass away with other ephemeral caprices, we may expect, in the course of a few years, to see the time arrive when an Englishman need not feel a sense of shame whilst conducting a stranger through the capital of his country.

De Plantarum, præsertim Cryptogamicarum, Transitu et Analogiâ, Commentatio. Autore Theophilo Gulielmo Bischoff. 8vo. Heidelberg. 1825.

The author of this work is one of a modern school of naturalists who maintain that all nature is divisible by some definite number, and that the many schemes that have been invented for classing natural beings, with a view to discovering the mutual relation which they bear to each other, must be tried by their conformity to this fact, which thus becomes the "lapis Lydius" of natural history.

We believe that these ideas, in modern times at least, originated with Dr. Oken, professor of natural philosophy at Jena, who, in his *Lehrbuch der Naturphilosophie*, published in 1809-10, after dividing plants into two great divisions, viz. *Cellulares* and *Vasculares*, finally declares, that, for physiological reasons, the number four is that to which all the divisions of the vegetable kingdom are reducible. As the opinions of Oken are little known in Great Britain, and as they evidently form the basis of subsequent similar botanical speculations, we shall preface the remarks we may have to make upon Mr. Bischoff's treatise, with a brief exposition of Oken's theory, which we entreat our readers to consider as seriously as it deserves.

According to this author, legitimate principles of the classification of plants can be derived from no other source than a consideration of physiological structure; or, as he expresses it, "of the organs of the anatomical system." Considered in this view, vegetation divides, firstly, into plants, of which the substance consists wholly of parenchyma (*cellulares*); and, secondly, into those provided with woody fibre in addition (*vasculares*). Of these, the cellulares are developed under the form of root, leaves, stems, and fruit; but the vasculares exhibit one higher degree of evolution, in the flower. Now,

as the peculiar characteristic of the cellulares is their fructification, and as the peculiarity of the fructification is its womanhood (*weibliche geschlechte*), it follows that the cellulares are female plants; and, as the peculiar characteristic of the vasculares is their flower, and of the flower its manhood (*männliche geschlechte*), it is equally clear that vasculares are to be considered male plants. But further, as the idea of sexuality depends upon a double essence (*zweiheit*), an organ which is only of the female gender cannot be rightly considered of any sex whatever, wherefore the cellulares, or female plants, are asexual; and as it is a known natural phenomenon that no male organs exist in plants without female ones, it is a necessary conclusion that vasculares, or males, are in fact the only plants which can be called sexual. Whence we arrive at the deduction that the kingdom of vegetation is divided into two regions: 1. That of *asexuality;* 2. That of *sexuality.*

Our investigation being brought to this point, the next thing to consider is in what way the particular developement of each of these is manifested externally. This the Professor finds to depend, firstly, on the root, which is independent of the operation of light; and, secondly, on the stem, leaves, and flowers, which are acted upon continually by the operation of light.

Now, as these are the four most important organs of plants, Oken declares that they must necessarily constitute the basis of classification; and as those plants which do not depend upon the action of light are asexual, while those that do depend upon it are sexual; and as there is *one* kind of influence apparent in the former, namely, that of the root, and *three* in the latter, namely, that of the stem, leaves, and flower, we are led to the inevitable inference that the natural divisions of plants are *four*, viz.:

1. Root-plants (*Wurzelpflanzen*),
2. Stem-plants (*Stengelpflanzen*),
3. Leaf-plants (*Laubpflanzen*),
4. Flower-plants (*Blumenpflanzen*):

of these the root-plants answer to *Acotyledones;* the stem-plants to *Monocotyledones;* the leaf-plants to *Dicotyledones apetalæ;* and the flower-plants to *Dicotyledones monopetalæ* and *polypetalæ.*

Each of these forms a circle, which, in its turn, is affected by the four above-named physiological influences, whence every circle is divided into four others, which are, in their turn, again sub-divided upon the same principle.

Such were Oken's notions in 1810; his more recent works have not fallen in our way, but we believe they still remain essentially the same. They seem to have been founded upon Goethe's opinions respecting vegetable metamorphosis, and to be, in fact, an attempt to apply the ideas of that most original thinker to the purposes of systematical arrangement, or rather, perhaps, to reduce the practical arrangements already existing to his own peculiar method.

It was naturally to be expected that a doctrine so abstruse, and even unintelligible as this, would be slow in making progress in the world; accordingly we find, that for a long time, it excited attention in no other countries than Germany and Sweden. In the former, the well-known work upon Fungi, by Nees von Esenbeck, may be cited as an example; and in the latter, the writings of Fries. The publication of Nees, however, applies to a very limited portion of botany, and differs materially in many respects from the principles propounded by Oken, while the writings of Fries may be said to be in some degree an adaptation of them. It is extremely difficult, however, to comprehend the manner in which this author views the subject, and to ascertain whether he adopts the theory of Oken, that the number four is the necessary consequence of the predisposition of vegetation to develope itself under four different forms, or whether he merely determines that four is really a number into which the lower orders of vegetation are distributable.

A few years since, an English zoologist, of high reputation, during his investigation of the natural affinities of insects, also arrived—without any acquaintance with the works of the above-named authors—at the conclusion that there is some certain number that prevails in nature; but the number upon which he fixed was five, not four. It is needless to add, that this naturalist was Mr. W. S. Macleay. This doctrine would, perhaps, in less skilful hands, have found but few followers; but, supported by the talent and practical knowledge of its author, it has been adopted, either wholly or in part, by a considerable number of the zoologists of this kingdom. It seems to us that this supposed discovery was not the consequence of any preconceived ideas, nor the result of any opinion of the necessary existence of some definite number like that of Oken, or perhaps of Fries; but that its claim to credit rests upon an ascertained certainty of the existence of this ultimate fact, which is not to be accounted for by any known law. If this view of the subject be correct, the credibility of the system must depend, not so

much upon abstract reasoning, as upon the force of evidence and assertion by which it is supported; for no reason is assigned by Mr. Macleay, at least, for the prevalence of one number rather than of another, if there be by Professor Oken.

It is necessary here to observe, that one of the most efficient means which has been employed to support these opinions, both in this country and abroad, forms the subject of the work at the head of this article; namely, the distinction between the affinities and analogies of animated beings. Since much of the evidence which has been adduced in favour of the doctrines in question depends upon a due understanding of the ideas expressed by these two words, it is proper that their respective value should be attached to each. It is assumed that the first systematic division of which beings are susceptible is into two series, agreeing with each other in general circumstances, and being, in *analogy*, the component parts of each series according in particulars and being in *affinity* with each other. The relations of analogy are ideally represented by two lines running parallel with each other; and those of affinity, which Mr. Bischoff calls transition, by circles connected with each other in inextricable entanglement. Such is the distinction attached to these terms by those writers who use them: how far such ideas may be vague, or susceptible of strict application, may not perhaps be quite so evident. As we presume that, however mystically they may be explained, their plain meaning is, that *analogy* signifies *remote affinity*, and *affinity*, *immediate analogy*, it does seem to us, that cases must be continually occurring in which the terms are convertible, and that their value must be altogether influenced by contingent circumstances. Thus, for example, the genera Berberis and Bocagea are, we presume, in analogy if considered with reference to Berberideæ, and Anonaceæ, but in affinity if viewed as a part of Thalamifloræ; so again Aroideæ and Aristolochiæ, are in analogy with respect to Monocotyledones and Dicotyledones, but in affinity as to Vasculares. Such, however, is the distinction upon which much of the truth of numerical divisions in nature depends; but that we may not intentionally misrepresent the matter, we will give a definition from Mr. Fries.

"*Affinia* igitur sunt quæ in eadem serie sequuntur, et in se invicem transire videntur. Hæc in ulterioribus congruunt, sed in citerioribus rationibus differunt. *Analoga* autem dicimus quæ in diversis seriebus locis parallelis posita sunt et sibi invicem correspondent. *Syst. myc.* 1. xvi."

Having prefaced thus much, we may proceed to a consideration of the value of the systematical opinions in question. The three heads under which they can be most usefully examined are, 1. As to their practical accordance with nature. 2. As to their accordance with each other. 3. As to the external evidence by which they are supported, independently of the classification of their authors.

As to their practical accordance with nature, it is worthy of remark, that the botanists who have adopted Oken's ideas, have confined the application of them to the lowest forms only of vegetation. Of these plants the structure is so extremely simple, consisting only of cellules in various states of juxtaposition, that it is not to be doubted that they may be adapted to almost any system that can be invented. The arrangement of their moleculæ is believed by some to be influenced in a high degree by the four elements; hence it is possible that their tribes may be particularly well fitted for falling into a quaternary distribution. This opinion seems to be that which led Nees to the application of a quaternary arrangement to Fungi. But if it be admitted, that, for certain physical reasons, a division by four may possibly exist among the simplest forms of vegetation, it does not necessarily follow, that plants of a higher degree of evolution are subject to the same law. Indeed Fries himself seems to be of this opinion, when he says, "in superioribus sphæris ob organa plura major (numerus) admittendus quam in inferioribus. *Plant. homon.* 21." He may therefore be supposed to have adopted Oken's system only partially, and in its general principles, and not with the intention of applying it to particulars.

With respect to the system of the latter writer, as his plan is applied to the whole kingdom of vegetation, and as his ideas, with whatever peculiarity they may be expressed, are not to be misunderstood, they present a fairer mark for criticism. We have seen that, to obtain his four divisions, he sets out with the assumption that the four degrees in which four different parts of plants, viz. the root, stem, leaves and flower, are respectively developed, necessarily form the basis of any philosophical system; for to be philosophical it must depend upon the immutable workings of nature as developed in generals, not in particulars, which he finds to depend upon the four parts in question. Now this doctrine arises out of certain abstract ideas of vegetation which prevail extensively among the philosophers of

Germany, but which we believe we do not err in stating to be wholly unknown here. Oken and his followers maintain that a plant is to be considered as *organized water*, with two poles, one of which is directed to the heavens or light, the other to the earth, with a strong preponderance towards the latter; one pole being telluric, the other celestial. The elements of this organized water, they say, are two, cellular and vascular, both acted upon, not only by the polarity of the individual they constitute, but also by the influence of water and light. The root and stems they understand to depend upon a telluric agency, to be the same in essence but different in station, the root being a stem under the influence of water and earth, and the stem a root under the influence of light and air, and the leaves and flowers to depend upon a celestial agency, to be also essentially the same, but modified by circumstances, the leaf being specifically influenced by air, the flower by light. It is these opinions which have given rise to Oken's mode of arrangement in fours, which does not appear to have been considered even by himself reducible to practice, but rather a speculative view of the mutual relations of vegetable bodies. To show, however, by one example, that whether we understand the intentions of the author rightly or not, his method is, in fact, with regard to the characters he builds it upon, wholly at variance with nature, and totally inapplicable to practice, we will take his definition of *stem-plants*, which answer to *monocotyledones*, and see what it is worth. In stem-plants, he says, the leaves and flowers are kept back, a flower can be seldom found perfect in both its calyx and corolla, and the vascular system is but imperfectly communicated to the leaves. (*Lehrb.* 1507.) The leaves being kept back, that is to say, imperfectly formed, evolved or developed, is the first proposition. What! is the wide-spreading coma of the Palm and Banana, or the dense herbage of the Grass tribe, or the rigid foliage of Pandaneæ and Bromeliaceæ, all stem-plants, and most numerous in their kinds, are these instances of imperfectly developed leaves? To the leaves of some of the palm tribe it is well-known that those of no other plants can be compared for excessive developement. But, says Mr. Bischoff, these are not leaves at all, they are "expansiones tantum foliaceæ ipsius trunci substantiæ habenda"—mere foliaceous dilatations of the trunk, *analogous* only to the true leaves of leaf-plants and flower-plants, but by no means to be confounded with them, as has indeed been

declared by Oken himself, who calls the foliage of his stem-plants *blatt*, while he distinguishes other leaves by the name of *laub*. Unfortunately, however, the difference which these gentlemen suppose to exist between leaves and leafy dilatations absolutely amount to nothing. Undoubtedly there is an apparently different distribution of the veins of the *blatt* and the *laub*, those of the former running in nearly parallel lines from the base to the apex of the *lamina*, while those of the latter have a reticulated appearance; but this seeming difference depends upon nothing more than the angle at which what Mr. Lindley calls *venæ primariæ* diverge from the axis of the leaf, and is accompanied by no physiological distinctions. The function, insertion, and whole economy of the leaves of the *stem-plants* are, as far as we know, identical with those of other leaves, and besides, this peculiar mode of formation is not confined to them, but exists equally in *Melastomaceæ*. But, says Mr. Bischoff, the leaves of the stem-plants form one mass with the trunk, and are not separated from it by any articulation. This is false in fact,—the leaves of both Orchideæ and Cycadeæ being articulated with their stem. What then becomes of the first characteristic of this kingdom of Professor Oken? The flower is also said to be kept back; the flower of the Liliaceæ, and Amaryllideæ the most fully developed in all nature, to be imperfectly evolved; those of Palms, the mass of which frequently weighs an hundred weight, to be kept back. Surely nothing need be said upon this point. As to the supposed amalgamation of the calyx and corolla, if true, this would form no ground of distinction, for plenty of instances of the same kind are to be found in other plants; we are told, however, that the flowers of stem-plants are only *analogous* to those of other vegetables. The last proposition we have to consider is, that the vascular system is but imperfectly communicated to the leaves. As this is a repetition of the first proposition in another form, we might dispense with dwelling upon it, if it did not seem to show yet further the utter worthlessness of Oken's whole definition of stem-plants. It is said that the vascular system is but imperfectly communicated to their leaves; and yet it is remarkable enough that it is the abundance and perfection of this very vascular system in these plants that gives tenacity to the leaves of various grasses; that supplies the coarse cordage obtained in the tropics from certain Palms; that forms those beautiful handkerchiefs, emulating the finest muslin, which are manu-

factured from the Musa textilis; and, not to multiply examples, where examples abound, it is this same vascular system which, according to Oken, should scarcely be found in his stem-plants, that has rendered the flax-plant of New Zealand an object in some degree of national importance.

Such are the grounds upon which this much-extolled quaternary system of plants has been founded by its author. Such is the basis of that system of Fries, to which Mr. Macleay has appealed in support of his own quinary doctrines.

With regard to the opinions of Mr. Macleay, as they have not been applied in botany, they ought not, perhaps, to be criticised in this place, but they are too closely connected with this subject to be omitted entirely. Mr. Macleay has confined his system of quinary divisions to zoology, and of its applicability to that science we cannot pretend to form a judgment; but with botany we may be permitted to express our opinion that it has no relation. It has been admitted that it would be difficult to invent a system which, in skilful hands, might not be applied to the lower orders of plants; our remark, therefore, refers to vegetation in its extended sense; and this being understood, we know of no one fact in botany which tends, even remotely, to invalidate this opinion. So much space has been already occupied with this subject, that we want room for more extended arguments; there is one point, however, in which Mr. Macleay has called botany to his aid, upon which we must remark. In the Horæ Entomologicæ,—we have not the work at hand, and speak from memory,—it is asserted that there is a direct *analogy* in nature between the vertebrata and annulosa of animals, and the dicotyledones and monocotyledones of plants, the former of each having their skeleton in their axis, the latter of each having their skeleton in their circumference. This is altogether an error; no such analogy exists. It is true that there is a material difference in the structure of the stem of monocotyledones and dicotyledones, but not of the nature assumed. The skeleton of dicotyledones, by which of course the woody fibre is intended, is not central like that of vertebrata, but intermediate between two bodies of parenchyma, or flesh, by which it is also traversed in rays: here, then, there exists no analogy. The skeleton of monocotyledones is not external like that of annulosa, but consists, figuratively speaking, of an infinite number of bones, which branch upwards through the flesh, which is always external. Here also is no analogy. The

supposed analogy between animals and vegetables is, therefore, as far as Mr. Macleay's system is concerned, an absolute fallacy. That of Professor Agardh we will quote, and leave our readers to form their own opinions of the *analogies* discovered by it. Dicotyledones, this ingenious botanist discovers to be analogous to mammiferæ; monocotyledones to birds; ferns and mosses to amphibia; and the lowest orders of vegetation to fishes!

With respect to the accordance of the various opinions that exist in nature, reducible to some definite number, it is scarcely possible that any systems can be more discordant. We have seen that Oken and Fries maintain the number *four;* Mr. Macleay, who is followed by several experienced naturalists, contends for the number *five;* Mr. Kirby, whose extensive knowledge of Mr. Macleay's own ground cannot be disputed, seems to incline to the number *seven;* while Mr. Virey is said to prefer the number *three*; to say nothing of those who think a binary or dichotonous mode of division is the true one, thus dignifying the commonest principles of analysis with the name of system. Mr. Macleay, indeed, attempts to reconcile his ideas with those of Fries, by assuming that the latter really used a quinary mode of arrangement under the semblance of a quaternary system; bnt this Mr. Fries, in a recent work, expressly denies, disclaiming all disposition to attach *cabalistical virtues* to any number whatever. Indeed, without such a disclaimer, it is evident, after what we have seen of Oken's system, upon which Fries certainly builds, that it is quite impossible that the number five could have entered into his contemplation.

Of the external evidence by which these systems are supported, we have little necessity, and less space to treat. Professor Agardh, who evidently leans to the opinion of Oken, is obliged to confess (*Syst. Alg.* xii.) that he cannot reduce the Algæ, his own peculiar tribe, to four divisions, and that, on the contrary, six is the number to which he finds these plants reducible. So through the whole of his Aphorisms, and even his latest work, the Classes Plantarum, it is obvious that he finds it impossible to divide vegetation by any certain number whatever. Mr. Macleay's reliance upon the system of Fries, in support of his notions of quinary divisions, we have already seen to be unsupported; so is his appeal to De Candolle. That botanist has, indeed, established five principal tribes of Cruciferæ, the number of which, by the way, is already altered by the discovery of

Schizopetalon, but division by five goes no further even in that order; and it is a remarkable fact, that in all the other parts of the volume in which the supposed coincidence with Mr. Macleay's views are found, there is no other case in which any tendency to a quinary division is discoverable. In Berberideæ the genera are six; in Podophylleæ the tribes two, and the genera five; in Nymphæaceæ, the tribes two and the genera four; in Papaveraceæ, the genera nine; and in Fumariaceæ, the genera are six. In his first cohort of Thalamifloræ, the orders are eight, and in the second they are four; thus telling in favour of Oken rather than Macleay.

But to conclude: we hope that what has been here said will be viewed with no feelings of asperity, but as the legitimate reasonings of fair and temperate criticism. If we cannot agree with the naturalists whose opinions we have discussed, it is not from any want of a due sense of their scientific attainments, for we have the highest respect for all of them, but from a sincere desire ourselves to steer clear of delusion, however brilliant it may be, and to guard the world from its influence by such means as we may chance to possess.

Note on the Chemical Composition of two Liquids lately proposed as Disinfectants *of great power; and on the action of those Liquids on putrid Animal Matter.* By A. B. Granville, M.D., F.R.S., &c., Physician in ordinary to His Royal Highness the Duke of Clarence.

[Read before the Royal Society, February 20, 1827.]

A chemico-medical discovery, which bids fair to prove of the highest service to society, has for some time engaged the public attention in France, and is now making its way into this country. It consists in an improved method proposed by Mons. Labarraque, a *Pharmacien*, residing in Paris, of disinfecting air in which putrid animal effluvia are diffused, by means of chlorine, in a liquid form, instead of the troublesome and inconvenient process recommended by the late Guyton de Morveau, of evolving that gas in the dry state, from a mixture of manganese, marine salt, and sulphuric acid.

Mons. Labarraque's improved method consists in employing either a solution of the bleaching powder (the old oxy-

muriate of lime) in water; or a solution of crystallized carbonate of soda, likewise in water, into which a large proportion of chlorine has been introduced. He seems to prefer, and I think with justice, the latter preparation, although he has employed both with nearly an equal success.

The numerous and important applications that have been made of these two liquids in France, for the last three years, deserve the most earnest attention. By their aid, the putrefaction of dead bodies, though far advanced, has been effectually arrested; and the preservation, for a considerable length of time, of dead bodies that had not yet undergone the putrid fermentation, has been easily accomplished. The purification of the tainted air in hospital wards, prisons, workhouses, lazarettos, ships, sick-chambers, closets, sewers, wells, drains, stables, and manufactories in which animal substances in a state of putrefaction are used, has been effected by the liquids in question, in the most satisfactory manner; and not only with greater ease, but in a much shorter period of time than by any other method previously employed. Subsequent experiments have shown that these liquids may also be used with great advantage in arresting mortification or gangrene in the living body; and, it is probable, that further applications of the utmost interest may shortly be published.

In the hands of the physician, whose duties call him to the bed-side of patients labouring under infectious or contagious disorders; in those of the anatomist, whose pathological inquiries expose him to much risk and inconvenience, or who is eager to preserve free from putrefaction the objects of his research; to the magistrate who has the salubrity of large and crowded communities intrusted to his vigilance,—the disinfecting liquids have already been of the most essential service, accomplishing their respective objects with a success that no other method previously devised had ever ensured.

These are facts that rest no longer on the mere *dictum* of a single individual. They are attested by hundreds of persons, and are held as indisputable. The repeated experiments which I have had occasion to institute respecting many of those facts have only served to corroborate them; and the result is, that the disinfecting liquids are now generally employed on the

Continent for all the purposes just enumerated, under the immediate auspices of government; and it is to be hoped that they will also become equally known, and as extensively used, in this country.

Hitherto, however, the manner in which the discovery has been noticed or explained in England, has tended more to retard its progress, than to make it familiar to the public. Added to the natural difficulty of rendering correctly into English the chemical names of the French author, there have been still greater difficulties raised by adopting, as equivalent in English, denominations which have a very different import. The original fault lies with the discoverer,—the subsequent ones with his commentators and translators.

Mons. Labarraque assumes that when chloride of lime (*chlorure de chaux* of the French) is dissolved in water, and in that state kept for use, the integrant composition of that salt remains ever the same. Hence, to his first disinfectant he has given the name of *solution of chloride of lime.* Induced, most probably, by considerations of some pretended analogy, Mons. Labarraque takes it for granted that when *chlorine* is introduced into a solution of carbonate of soda, a *chloride of soda* (or as he calls it a *chlorure d'oxide de sodium*) is formed, and nothing else; and for this reason he has applied the latter denomination to his second disinfecting liquid.

Nowhere has Mons. Labarraque made it appear, that he had taken steps to ascertain, by actual analysis, how far his views were correct in either, but more especially in the latter case; and it is somewhat surprising, that notwithstanding the total absence of such evidence, every one who has had occasion to speak of, use, or report on the remarkable properties of the two liquids in question, as well in France as in England, has implicitly adopted Labarraque's names, and with them, of course, his theoretical notions respecting their chemical composition.

Attracted by the importance of the applications of Labarraque's liquids, I was led some months ago to examine how far his theoretical views were correct.

With regard to the composition of the first of the disinfecting liquids, said to contain chloride of lime, it is not my intention to speak at length. That subject has been well and properly

discussed, more than once, by some of the first chemists of the age; and there can be no doubt, that, after a while, no such thing as a simple chloride of lime is to be found in the water in which that compound has been dissolved; but that other salts are formed from its decomposition and probable action on the water, and that, therefore, the name given to this disinfecting liquid, by Mons. Labarraque is, to say the least, exceptionable.

My object, in the present paper, is to prove by analysis, that the second, and by far the most important, of the disinfecting liquids does *not* contain a chloride of *the oxide* of sodium, as advanced by the French author, and that, consequently, the name he has given to that liquid is inaccurate.

This, however, is not the only purport of my present communication to the Royal Society. Having had occasion, for the last fifteen months, to use very extensively the disinfecting liquid of soda, I have been able to ascertain certain facts which seem to have escaped the attention of the discoverer, as well as of his commentators, and which I cannot but consider as of the utmost importance in the history of Labarraque's discovery; inasmuch as those facts afford the only satisfactory explanation of the *modus operandi* of that liquid in destroying animal putrefaction; bring to light new compounds; suggest a method of detecting and fixing putrid animal effluvia existing in the air, applicable in times of infectious disease; and, lastly, must lead to the adoption of less troublesome and cheaper modes of preparing the disinfecting liquid. These facts it is my intention to submit to the Royal Society.

That a chloride of oxide of sodium was *primâ facie* not to be expected in Labarraque's second liquid, was to be presumed on two grounds; first, because the existence of that compound is still considered as problematical, by the majority of the leading chemists, especially when so large a quantity of soda is present in the solution, as in this case; and secondly, because mere reflection on what takes place during the process of preparing the disinfecting liquid of soda, according to the author's own formula, suggests the improbability of such a compound being formed.

But as both these grounds of doubt might be looked upon as

subjects of mere controversy, I undertook to settle the question by actual experiments, both analytical and synthetical: using, for that purpose, the disinfecting liquid of soda prepared agreeably to the directions of the French author in the following manner:—

Twenty fluid ounces of distilled water containing each 437.5 grains, were made to dissolve one-fourth of their weight of pure crystallized carbonate of soda, and chlorine gas was passed into the liquor to a complete saturation of the latter.

This is the so-called solution of chloride of oxide of sodium of Mons. Labarraque, his commentators, and translators; and possesses, to an eminent degree, all the disinfecting properties first ascertained by that gentleman.

This liquid has a specific gravity = 1.064, and contains 572.66 cubic inches of chlorine gas. It is of a bright pale yellowish colour, perfectly transparent, smelling moderately of chlorine gas, having a very pungent taste, and producing cough and constriction of the throat when swallowed undiluted. Being put into bottles, immediately after its being prepared, it effervesces rapidly for a short time on removing the stopper. This phenomenon is not readily observed during cold weather, and although it be owing to the escape of some of the chlorine gas, with a very minute portion of carbonic gas, that had lurked behind during the operation of preparing the liquid, it does not seem to influence, in any sensible manner, the properties of the liquid.

Chemical Composition of the Liquid.

A. The twenty ounces of liquid just described were evaporated to dryness, on a sand-bath, at a temperature often exceeding a little that of boiling water; towards the end, chlorine gas escaped, and continued to do so until *almost* all moisture had disappeared. The dry residuum weighed 725 grains: its appearance was that of a perfectly white salt, amorphous, granular, each minute grain as distinctly separate from the other as in fine dry sand, neither efflorescent nor sensibly deliquescent. It has a marked taste of muriate of soda, with a peculiar, but slight degree of pungency, reminding one of the taste and smell of chlorine, but quickly vanishing, and leaving behind the simple taste of common salt.

B. One hundred grains of the saline residuum A. were dissolved in distilled water. The liquid was transparent and colourless, having the same degree of taste of marine salt which the dry residuum had, and being slightly styptic. Its specific gravity was found to be $=1.054$. It contained no carbonates, emitted no smell of chlorine, and had lost all *disinfecting* as well as discolouring properties.

Treated with nitrate of silver, it gave a copious precipitate of muriate of silver, which, when filtered, washed, and dried, weighed 180 grains, corresponding to 73.53 of muriate of soda, or chloride of sodium. The precipitate, by another experiment, was ascertained to be soluble in ammonia.

The nature of the solid contents remaining in the solution after this part of the analysis, might properly have been *inferred* to be chlorate of soda; since it is known, according to Thenard and others, that chloric acid is formed, whenever a solution of an alcali is placed in contact with chlorine. But, in order to prove that such was the case in reality, the filtered liquid was slowly evaporated, and by two successive crystallizations the nitrate of soda was separated, leaving a salt which, when dried, weighed 26.47 grains, and presented every indication of being a neutral *chlorate* of soda.

Another experiment was made to confirm this conclusion.

C. One hundred grains of A. were repeatedly digested, agreeably to Chenevix's process, in strong alcohol. By this means, the muriate was separated from the chlorate, and the latter obtained in a comparative state of purity.

D. To obtain a still greater degree of accuracy in the above results, a fresh solution of 100 grains of A. in water was prepared, and afterwards decomposed by sulphate of silver, which gave the same proportion of the muriate, or chloride of sodium, above stated. The sulphuric acid was got rid of by barytes; and the remaining salt presented, as before, the character of chlorate of soda.

By another experiment, the latter salt was decomposed, and the whole converted into a chloride of sodium. For this purpose:—

E. A stream of ignited gas, under a pressure of ten inches of water, was directed on a small portion of the mixed salt A., on

a platina disc. The flame of the hydrogen became enlarged to about four times its natural volume. It gave a most brilliant and vivid light, which had a bright yellow colour, like that of chromate of lead. The combustion was accompanied by a whizzing and decrepitating noise. Water was formed, and the residuum was fused, and tasted like very pure rock salt; and was, in fact, dry chloride of sodium.

A synthetical experiment was now made, in order not to leave the smallest vestige of doubt on the subject.

F.—73.53 grains of muriate of soda, perfectly dry, and 26.47 grains of chlorate of soda, carefully prepared, were dissolved in distilled water. The liquid was found to be similar, in every respect, to that obtained and analysed in experiment B. It had the same specific gravity, and when submitted to the like agents, it gave precisely the same results. It possessed neither disinfecting nor decolorizing properties.

From these experiments, it follows, that not only does the liquid of Mons. Labarraque, which we are considering, contain *no* chloride of oxide of sodium; but also that the presence of the salts of chlorine which it really contains is insufficient to produce those singular results on animal effluvia, and vegetable colours, which render the liquid itself so valuable; and it further appears that those singular results must be due to some principle which is lost during the evaporation of the liquid.

That the principle in question must be *chlorine* is quite evident; and therefore it is to the agency of that uncombined principle alone, that the properties of the disinfecting liquid of soda are to be attributed, and not to the presumed existence of a chloride of oxide of sodium in it.

To prove this position, the following experiment was made.

G.—A fresh portion of the dry residuum A., amounting to 145 grains, was dissolved in four ounces of distilled water. This liquid was found, as already observed, without effect, either on putrid animal effluvia or on vegetable colours. 13.86 cubic inches of chlorine gas (being double the volume of the water) were now introduced at the common temperature of 50° of Fahr.; when all the peculiar properties of Labarraque's disinfecting liquid in its original state were immediately produced.

The same results followed when the same quantity of chlorine gas was introduced into a like quantity of the artificial mixture of muriate and chlorate of soda mentioned in experiment F.

Now, unless it can be proved by equally clear experiments, that the introduction of chlorine into a solution which contains a muriate and a chlorate of soda converts both salts into a chloride of an oxide; or, in other words, unless it can be shown that an excess of chlorine in such a liquid, holding a muriate and a chlorate of soda in solution, as demonstrated by the preceding analytical and synthetical experiments, decomposes the two salts in question, and setting their acid free, seizes on their oxydized bases to form the pretended chloride of an oxide, the conclusion we must arrive at from the present inquiry is inevitable; namely, that the disinfecting liquid of soda of Mons. Labarraque is not what he considers it to be, but a mixture of

Chloride of sodium . .	73.53 grains
Chlorate of soda . .	26.47 grains
	100.00

with an excess of chlorine.

Or in 20 oz. avoirdupois of the liquids,

Chloride of sodium .	533.09 grains
Chlorate of soda .	191.91 grains

with free chlorine equal to 69.30 cubic inches.

That such must be the constitution of this liquid, is made further manifest by the application of the atomic doctrine to the experimental process followed in preparing it.

Five atoms of oxygen whichc can only proceed from so many atoms of soda, are required to form chloric acid; for which purpose they combine with one volume of chlorine. The result is an integrant atom of the acid, which can only combine with one atom of soda to form the chlorate. On the other hand, the five atoms of sodium, deprived of their oxygen, combine with as many atoms of chlorine, and form five integrant atoms of chloride of sodium. Unless we have recourse to the decomposition of water, it is impossible that any other combination or arrangement than the one just stated can result from the action of chlorine on soda in this case.

Now, if we take the weight of the five atoms of sodium to be (according to Dr. Thomson) = 15 and that of five atoms of chlorine to be . = 22.50

The resulting chloride of sodium = 37.50
while the weight of five atoms of oxygen, and that of one atom of chlorine to form chloric acid = 9.5
And the one atom of soda to be . = 4

The neutral chlorate will be . = 13.50

Hence we find by the atomic theory, the results given by actual experiments to correspond, with singular precision, with the theoretical numbers: for 37.50 is to 13.50 as 73.53 is to 26.47, the respective weights of the chloride of sodium and chlorate of soda in 100 grains found by actual analysis.

Another striking proof of the precision of that analysis, is derived from the consideration of the prodigious quantity of chlorine which enters into the composition of this disinfecting liquid.

One hundred grains of the mixed salt constituted, as shown by the preceding experiments and atomic proportions, contain 52.94 grains of chlorine, corresponding to 69.43 cubic inches of that gas; consequently, as 725 grains of that mixed salt were obtained from twenty ounces of Labarraque's liquid, according to experiment A., it follows that 503.36 cubic inches of chlorine must have been absorbed to form them.

But, as 572.66 cubic inches of that gas were actually thrown into the twenty ounces of liquid; the difference can only be accounted for by supposing that some of the gas was held in a free state in the solution; and it is curious to remark that the difference in question (amounting to 69.30 cubic inches) is precisely double the bulk of the water employed in preparing the liquid,—a fact which, while it agrees perfectly with the received notion of the absorbing power of water with respect to chlorine, serves, at the same time, to confirm that notion.

Now, the 503.36 cubic inches of chlorine employed to form the 725 grains of the mixed chloride of sodium and chlorate of soda in the twenty ounces of liquid, weigh, according to Dr. Thomson's tables, 383.815 grains, while the soda so employed, according to the atomic theory, weighs 341.185 grains.

Hence we have here, as before observed, a third beautiful confirmation of the correctness, numerical as well as experimental, of the present analysis. For

Weight of chlorine = 383.815
That of soda . = 341.185

Total = 725.000 = equal to the weight of the solid contents in the liquid; in which the bulk of the chlorine gas to that of the water is as 16 to 1 exactly.

It may, perhaps, appear to the members of the Royal Society that I have attached rather too much importance to this subject by carrying my analytical inquiries to such a length: but when it is considered that the individual to whose ingenuity we are indebted for the application of the liquid examined (an application which we may boldly predict will, in a short time, acquire great popularity, while its value will be generally acknowledged) sent it forth to the world without one word of illustration or explanation as to the physical and chemical characters of that agent,—nay, coupled it with erroneous notions respecting its composition and chemical nature,—I hope to find an excuse for my prolixity in supplying those deficiencies, and thus counteracting the mischief they are already working in this country, by threatening, at this moment, to put an end to the discovery altogether, owing to the mistakes promulgated respecting it. One short example will better illustrate my position than any argument I might employ for that purpose.

The valuable properties of Labarraque's liquid having, through the means of the press, become extensively known, and those properties having been ascribed, by the same press, to a supposed chloride of oxide of sodium dissolved in water, demands have been sent, from various parts of the country and the metropolis, to Mr. Garden, an operative chemist (whom I have employed to prepare several gallons of the liquid for my use, and on whose accuracy I can depend), for large quantities of the solid contents of the liquid, as more conveniently transportable. What the result of the trials must be, which will be made with these solid contents, it is easy to foresee, when we consider that, by the experiments detailed in this

paper, it is proved that those solid contents are totally destitute in themselves of the valuable disinfecting and decolouring properties belonging to the liquid, and of which the persons making the demand are in search. Nay, it is a fact, that since the notice which has been taken in this country of Labarraque's liquid, through the public journals, a person has actually forwarded to London the pretended chloride of oxide of sodium in a dry state, so cheap as to undersell the metropolitan chemists; and that certain medical students, and others who used it, in full expectation of witnessing the striking and immediate results belonging to Labarraque's liquid were sorely mortified at the failure which attended their trials, and actually insist on proclaiming that gentleman's discovery to be an imposition.

This was to be expected, as the free chlorine contained in the liquid is the only disinfecting principle belonging to it, as has been already stated in the course of this paper. But, in order that the Society may judge with what justice it has been so asserted, I may probably be permitted to relate, shortly, two, out of several, comparative experiments which I instituted in order to prove that fact; and with them I shall conclude my present communication.

Some portions of muscles, and a quantity of membranes, were placed (July 26, 1826) in water, and under circumstances the most favourable for putrefaction. In a few days, the smell emitted from the deep jar into which they had been put was so strong that it was perceived long before entering the room. On stirring the water, the stench became insupportable, and the whole room was so soon and so thoroughly impregnated with it, that it was scarcely tenantable. I took out some of the muscles and membranes, which I found in an advanced state of putrefaction. I plunged them into some of the disinfecting liquid of soda diluted with four times its bulk of water, when they instantly lost their offensive smell, and, when applied close to the nostrils, gave out merely the odour of chlorine in a faint degree, and that of sound animal fat.

A second portion of the putrid muscles was similarly treated with a watery solution of the solid contents of the disinfecting liquid obtained by evaporation; but no change took place, and the putrid smell continued undiminished. Two measures of

chlorine gas were now introduced into the inert solution, and the same animal substances replunged into it afterwards, when all effluvia of putridity immediately vanished.

As a further corroboration of this curious fact, which, though completely overlooked by Labarraque, and all those who followed him, is, nevertheless, the main spring of the discovery, I tried the effect of distilled water holding in solution two measures of chlorine alone, when precisely the same disinfecting results took place on the putrid animal matter employed that had been observed in the first experiment; with this difference, however, that during the operation the escape of the chlorine gas was so considerable as to become insupportable to the bystanders and myself, producing cough, difficulty of respiration, and head-ache.

This difference in the mode of acting on the part of chlorine, placed under apparently similar circumstances, is curious, and requires explanation. What that difference depends on—what is the *modus operandi* of the disinfecting liquids on putrid animal matter—what the compounds are which are formed in consequence of that action—and how that action may be made available for the analysis of infected air;—lastly, what new and easier processes might be devised to obtain so valuable an agent, are points of great importance, which, after a laborious inquiry, I have succeeded in ascertaining, but which I must reserve for a future communication.

In the meanwhile, I think myself entitled to recommend the abolishing of the incorrect name applied to the liquid under consideration, by the discoverer; and to propose that of "Disinfecting liquid of Soda" instead; or, when prescribing in Latin, a denomination that shall record the name of the inventor, such as "Liquor Labarraquii chloro-sodaicus."

Extract of a Letter from Captain Sabine to Professor Renwick, of Columbia College, New York, respecting the Report on the subject of the Weights and Measures of the State of New York, printed in our last number.

"In consequence of my absence from England, I did not see your report on the subject of the weights and measures of New York, as you intended I should have done, before its publication in the last number of the Institution Journal.

"I am still, however, in sufficient time to transmit you the accompanying correction of the length of the pendulum determined by me at New York, so as to arrive before the meeting in September of the Commissioners for revising the laws of the state of New York; when your report, recommending a reference to that determination, as the means of identifying and defining the standard of linear measure of the state of New York, is to come under consideration.

"The correction arises from the value of the divisions of the level of the small repeating circle, which I employed in ascertaining the rate of the clock during the experiments at New York, having been found on examination to be ten seconds, instead of single seconds as I had supposed them by the engraving on the scale. The observations detailed in the Memoir to which you have referred have in consequence been recalculated; and the following table exhibits both the former and the corrected results.

	Interval.	No. of Days.	Chronometer's Loss on mean Solar Time.	
			Pen. Exp. p. 127.	Recalculated.
			s	s
Sun, W. of Meridian..	Dec. 22 to Jan. 2	11	2,61	2,60
	,, 22 ,, 3	12	2,59	2,75
	,, 23 ,, 2	10	2,76	2,64
	,, 23 ,, 3	11	2,72	2,79
Sun, E. of Mer.	,, 24 ,, 2	9	2,59	2,79
α Lyræ, W. of Mer.....	,, 22 ,, 2	11	2,59	2,57
Rigel, E. of Mer.....	,, 23 ,, 2	10	2,67	2,76
Rigel, Sirius... E. of Mer. ...	,, 23 ,, 3	11	2,59	2,76
By the disappearance of Stars ... No. 1	,, 24 ,, 2	9	2,50	2,50
By the disappearance of Stars ... No. 2	,, 24 ,, 2	9	2,59	2,59
By the disappearance of Stars ... No. 3	,, 24 ,, 2	9	2,59	2,59
Mean; Chronometer's loss per Diem...			2,62	2,67 sec.

"Whence it appears that the chronometer, and with it the clock and the pendulums, the rates of which were gained by comparison with the chronometer, were going five hundredths of a second in twenty-four hours slower than they were

esteemed to have gone in the account of the pendulum experiments: and the number of vibrations in a vacuum, in twenty-four hours of mean solar time, at a temperature of 62°, should be of pendulum No. three 86113,14 instead of 86113,19, and of pendulum No. four 86122,73 instead of 86122,78, as given in the 9th column of page 236 of the Pendulum Experiments.

"An accidental error of far greater magnitude has occurred in the summary of the results in the same page, in taking 86118,48 instead of 86117,98, as the mean of the above numbers, representing the vibrations strictly comparative with the number 86159,79 at London. This error does not exist in the separate account which I drew up of the experiments at New York, and which is published in the transactions of the Literary and Philosophical Society of that city*.

"The number of vibrations at New York strictly comparative with 86159,79 at London, including the small correction for the level, is then 86117,94; and the length of the seconds' pendulum, in the room at Columbia College in which the experiments were made, 39,10107, presuming the length in Mr. Browne's house in London to be 39,13908 inches.

"The height above the sea of the room in which the experiments were made at New York being 67 feet, this length would require to be increased ·00025, in order to obtain the length of the pendulum vibrating seconds at the level of the sea, presuming the reduction to be proportional to the squares of the distances from the earth's centre. The discussion of Dr. Young in the Philosophical Transactions for 1819, had, however, shown that this proportion can be in no case just, requiring always to be diminished by a co-efficient, varying in regard to the form and density of the materials composing the elevation on which the experiments were made; and the reasoning of

* "A similar error occurs in the same summary, in taking the mean of the vibrations of the two pendulums at Hammerfest, 86216,09 and 86225,83, and which ought to have been 86220,96 instead of 86221,46; and the resulting pendulum 39,19468 instead of 39,19512. I mention this, because they are the only errors of consequence to correct, that have yet come to my knowledge in the account of my pendulum experiments, and I will beg you to do me the favour of correcting them in the copies in the libraries of Columbia College, and of the Literary and Philosophical Society; with one other, with respect to the Magnetic Dip at Ascension, p. 474, which is printed South, when it should be North."

Dr. Young has been recently confirmed by the experiments of Professor Carlini on Mont Cenis. The amount of the coefficient, and consequently the value of the addition to be made to the length of the pendulum determined by the experiments, in order to reduce it to what might be supposed to correspond to a pendulum vibrating seconds at the level of the sea, cannot be otherwise in the present state of our knowledge than a quantity *arbitrarily* assumed; and it would, therefore, appear to me preferable for the purposes mentioned in your report, to take the pendulum of the place of observation, being the direct result of experiment, in place of the pendulum at the level of the sea, and thus to avoid the introduction of an arbitrary, and therefore questionable, reduction.

"Should this view, on this particular point, meet your concurrence, the commencement of the second provision in your proposed legislative enactment would run thus—(Journal of the Institution, new series, No. I., pp. 114, 115)—'That the unit of lineal measure shall be the yard, as used in this State at the date of the declaration of its independence; and that for its more precise definition, and in order to its recovery in case of loss, it is declared (until the measure of the pendulum shall be transferred to some appropriate public building,) that the said yard has been found, by experiments made with a pendulum with a brass rod at Columbia College, New York, to bear to the pendulum of that place, vibrating seconds in vacuo, at the temperature of melting ice, the proportion of one million (1,000,000), to one million, eighty-six thousand, one hundred and forty-one (1,086,141).'

"It is worthy of remark, that the acceleration between New York and London, 41,85 seconds, which I trust is finally corrected, is within one hundredth of a second of 41,84 seconds, the number originally communicated in the spring of 1823 to the Literary and Philosophical Society of New York, as the result of my experiments, and printed as such in the volume of their Transactions. It is almost unnecessary that I should notice, that a difference of one hundredth of a second in twenty-four hours is not a significant quantity in the proposed legislative proportion of 1,000,000 to 1,086,141."

Further Observations on the Use of the Gelatin of Bones as an Article of Food; addressed more particularly to the Friends of the Greeks. By Charles de Gimbernat.

I CANNOT but feel anxious that a process which was successfully acted upon in the year 1814, with a view to furnish subsistence to the garrison and inhabitants of Strasburg, should now be revived and acted upon to a much greater extent, on behalf of the intrepid defenders of Greece. The Philhellenes have done much towards supplying the military and financial necessities of that country; they have sent arms, officers, engineers, artillerymen, surgeons, artisans, and money, but the most pressing want of all is, I fear, still very imperfectly provided for. Missolonghi, after an obstinate resistance, fell by famine; and of the fortresses that remain, not one, perhaps, is victualled to stand a protracted siege. Nor is the want of a sufficient supply of food, by any means, confined to the fortresses: the probability is, that dearth will soon become general throughout the country. Wars for independence are always of long duration. If left to themselves, there is little reason to suppose that the Turks will be able, within any assignable period of time, to bring back the Greeks to their allegiance, or consent to recognise their independence. Whether the great powers of Europe will interfere is doubtful; and still more doubtful is it in what manner that interference will be exerted, and with what success. In whatever way we consider this subject, the conclusion to which we must infallibly arrive is, that dearth and the diseases incident to dearth, will, for a considerable time, prevail in every part of Greece, unless provisions are sent there from foreign countries. For these, the Greeks must trust, in a great degree, to the sympathy and benevolence of the several Philhellenic societies. Now, the funds of these societies must, in the nature of things, be limited and precarious. It is essential, therefore, that they should economise their means to the utmost; that the relief first administered should be directed to meet those wants which are the most urgent; and that it should reach these people for whose benefit it is intended; and consequently, be little liable to misapplication by those subordinate agents through whose hands it must pass. Under this impression, I respectfully submit to these

committees, that they cannot make better use of a portion, at least, of the funds, placed at their disposal, than by preparing and dealing out to the Greeks the food which it is the object of this paper to bring under their notice; of all descriptions of food that can be sent to them, the most wholesome, the most nutritive, the most economical, the most portable, and the most easy to preserve.

The process of extracting gelatin from bones was resorted to for the relief of the poor, during the scarcity of 1816 and 1817, both at Geneva and at Munich.

In London, and many parts of England, bones fetch a high price, being used as manure; they are also used in refining sugar, and making sal volatile. At Paris, the price is as high as eight francs per cwt. In the greater part of Europe, however, they are thrown away as good for nothing. What I propose, therefore, is that the Philhellenic committees should make it their business to obtain an abundant and regular supply of this material, from all the great maritime towns, where they may be had, for the most part, for the mere cost of collecting. What an immense quantity might not be collected from the consumption of meat at Amsterdam, Rotterdam, Hamburgh, Antwerp, Marseilles, Genoa, Leghorn, Naples, Cadiz, Lisbon, &c., if depôts were established in those cities. The extraction of the gelatin might either take place on the spot, or the bones might be transmitted to a common depôt—say any one of the Ionian Islands. There are several reasons why Greece itself would not be a proper situation for such an establishment. I sent to Greece, some time since, instructions for conducting the process, but no notice was taken of them. A people unused to manufactures and the operations of chemistry are not likely to direct their attention to such subjects amid the din of arms, and in the presence of an invading enemy. Greece cannot supply so large a quantity of bones as would be wanted, nor has she at hand the acid necessary to decompose them. Though the process is exceedingly simple, yet it would meet with obstacles, and be liable to fall into discredit if conducted by inexperienced hands.

I do not mean that the bones would require to be subjected to any very strict examination before they were used. Bones

of animals which have perished by disease, afford a gelatin as little noxious as if it had been extracted from the bones of those which were killed in full health. The observations of M. Seguin, Dr. Halli, and Dr. Orfila, have established the fact that a substance which has passed through solution, in hydrochloric acid, is not only free from infection, but acts as an antidote to it. So thoroughly am I convinced of the truth of this position, that I should not scruple to eat gelatin procured by the chemical method, when the animal, from which the bones were taken, had, to my knowledge, died of a contagious disorder. This antiseptic property is, by no means, unimportant in an article intended for camps and barracks in a country in which fever is extremely frequent.

The average quantity of bone in animals, is in proportion to flesh as one to five. The average quantity of gelatin obtained from bones, by the chemical process, is thirty per cent.

According to registers kept at the Prefecture at Paris, the mean weight of an ox is six hundred and sixty pounds; hence its bones may be said to weigh one hundred and thirty-two pounds, and to yield forty pounds of dry gelatin.

In 1822, the number of horned cattle sold at Smithfield market, was 1,682,190. The weight of butchers' meat consumed in London is 171,251,196 lbs. per annum; which yields 34,250,238 lbs. of bone, and 10,275,070 lbs. of gelatin.

The weight of butchers' meat consumed in Paris is 83,474,432 lbs. per annum; which yields 16,694,886 lbs. of bone, and 5,008,464 lbs. of gelatin.

These calculations, however vague, will give some idea of the immense quantity of nutritious matter which is at present either wasted or made use of for other purposes than that of supporting human life. In these calculations above stated, those animals only are comprehended which are killed at the slaughter-house: to these, however, ought to be added all those which die a natural death; nor ought we to consider butchers' meat alone. The bones of pork, game, poultry, &c., ought to be taken into account also; and these, it is estimated, are equal to a fifth of the bones of horned cattle. I dare not speak of horse-bones, though gelatin made from these would, probably, be found as good as any other. There can be no doubt that

ten or twelve establishments of the kind I recommend might be conducted at little expense, and would furnish provisions to the whole of Greece.

Fresh meat will not keep in hot or close weather, and salted meat is apt, after some time, to engender scorbutic disorders. For this reason, Parmentier proposed that cakes of gelatin should be given to armies on the march, so that sick and wounded might not be left to starve. Mons. Proust highly approves this recommendation. What beverage, says he, can be more invigorating than a couple of cakes of gelatin soaked in wine? The most exquisite combinations of cookery are invented for the spoiled children of fortune; and shall nothing be done in the commissariat for the poor soldier who bleeds in the service of his country, and is exposed to the greatest sufferings? Governments ought, at once, to adopt the suggestion, for they buy articles of the best quality, at the lowest price, and would lose nothing by its adoption. However, it was found that the article, as then prepared, was too costly to be introduced into armies; gelatin made of meat, with bones, cost from 10*s.* to 13*s.* 4*d.* per pound; of meat, without the bones, from 16*s.* 8*d.* to 20*s.* per pound.

Gelatin made, by the chemical process, of bones only, costs at Paris, at present, two shillings per pound, but ought not to cost more than fifteen-pence or twenty-pence at the utmost.

The substance extracted from bones contains in an equal bulk more nourishment than any kind of provisions, which is of the highest importance when it is requisite to victual besieged places. A small vessel laden with gelatin would carry more food than a large vessel laden with other kinds of provisions, and it would not only better elude the observance of an enemy's fleet, but also disembark on shores which great vessels drawing much water could not approach. For the same reason, gelatin cakes are better for fortresses than gelatin alone; they contain more nourishment in the same compass. The cakes should be packed in chests or casks well closed, so as to protect them from moisture, rats, and insects.

The catastrophe of Missolonghi would, perhaps, have been avoided, had the food which the brave Miaulis carried with him in January consisted of gelatin and biscuit. Gelatin

cakes made from bones will keep much longer than those made from meat, if no salt, sugar, vegetables, or other deliquescent substances be used in the preparation.

The best portable soups are not obtained from the prime parts of the animal, but from those which are the least subject to corrupt, as membrane, tendon, gristle, the raspings of bones, horns, ivory, &c., and the seasoning is added only just before they are served up. Besides bones, the only article wanted would be muriatic acid; this may be had in abundance at any establishment where soda is separated from sea-water. Being a secondary product, the price of it is inconsiderable; and if the committees would undertake to manufacture soda themselves it would cost nothing.

Gelatin broths are as nourishing as meat broths, but have no flavour. Now, mere food is not sufficient; the palate must be satisfied as well as the stomach. Gelatin, when pure, is tasteless; it does not contain the aromatic principle called osmazome, which meat has. Broths of gelatin, therefore, require more or less seasoning. For this purpose, we may employ meat or vegetables, or an extract from either.

By gradual drying in the oven, one pound of meat is reduced to two ounces, which is an advantage in transport: but the only way of defending this meat from insects, is, by closing it up in his boxes upon the plan of Mr. Appert, an expensive process.

Vegetables may be preserved at less expense by being put up in bottles. There is still to be mentioned an article much more economical, and which is well deserving the attention of the committees, particularly in Switzerland. Fermented cheese, according to the experiments of Proust, contains osmazome in less quantity, indeed, than meat, but more abundantly than any other animal substance; and is, therefore, well adapted to give a relish to the insipid gelatin. Many proprietors of cattle in Switzerland and the Low Countries, who have shown on more than one occasion, the interest they feel for their suffering fellow-creatures in Greece, might, very probably, be induced to offer voluntary contributions of this kind of food, if its value were distinctly pointed out to them. Those who could not afford to subscribe money would make no scruple of sub-

scribing produce; and their patriotic feelings would be more readily reconciled to exporting the provisions which they had themselves raised, than to exporting specie for the purpose of purchasing provisions elsewhere. I do not wish, however, by any means that the committee should confine their supplies to the articles I have mentioned. I would have them furnish wheat, barley, and other grain, likewise in proportion to the means which they have at their command, and more especially biscuit, which is a most essential article. My idea, I repeat, is to substitute gelatin and cheese for a part only of the provisions to be sent out, and not for the whole.

The manner of making broths with dry gelatin is as follows:—The quantity intended to be employed is first weighed and then cut into small pieces, which are left to steep in water till the acid is completely gone; this will require ten or twelve hours. Hot water is better for this purpose than cold. To a third of an ounce of gelatin add three-quarters of a pint of water, and a sufficient quantity of vegetables and spices to give it a relish: put them on the fire to boil: add also a little dripping to produce an oily appearance on the surface. The relish will be further improved if the colour of meat broth be given, which is easily done by burnt sugar, or burnt onion, or baked carrot, or a bit of toast.

The broth made in this way, though less savory than if made of meat, is not less nourishing: when cool, it turns to jelly, which meat broths do very rarely, and may be used as a substitute for isinglass. The jelly is quite pure and tasteless. It forms a sauce with gravy, and with bitter almonds, blancmange.

Mons. Michelot, in a memoir published in the *Revue Encyclopedique*, tom. xiii. p. 19, recommends that gelatin should be put into vegetable broths, which afford little nourishment, and are apt, when taken in quantity, to turn sour on the stomach. He calculated that forty pounds of gelatin would be sufficient for 2000 half-pint basins of broth. The bones of butcher's meat consumed at Paris would furnish daily this quantity of broth to 600,000 persons, and the bones of pork, game, and poultry to 120,000 more.

Mons. Darcet proposed to use gelatin broth in the Rumford soups instead of water. The bones which pass through

the kitchens at Paris, according to his computation, would serve for 1,200,000 basins of Rumford soup.

My views are somewhat different. I do not aim at discovering the minimum of food necessary to the support of human life. My allowance would extend to an ounce of gelatin per day, which would be enough for two or three basins of vegetable broth, and a pound of bread. But my estimate so far differs from that of M. Michelot, that I should allow to 200,000 individuals what he would give to three times that number.

If to four pints and a-half of water, and one ounce of gelatin, be added eight ounces of meat, we shall have a broth as good in appearance and flavour as if it had been made with three times the quantity of meat. Thus, by the new process, there is a saving, not only when gelatin alone is employed, but also when (to have a broth possessing more relish) a fourth part of the meat which would be necessary in ordinary cookery, is added to a quantity of gelatin equivalent to the substance of the other three parts.

From a report delivered by the faculty of medicine at Paris, it appears that one hundred weight of raw meat will yield sixty-seven of roast meat, or fifty pounds of boiled, and two hundred basins of broth.

It appears further that one hundred weight of meat of which twenty-five pounds is made into broth with three pounds of gelatinous matter, will yield two hundred basins of broth, and twelve pounds and a-half of boiled meat, leaving seventy-five pounds, from which may be obtained fifty pounds of roast meat.

By this method, then, we get the same quantity of broth as before, fifty pounds of roast meat and twelve pounds and a-half boiled: the gelatinous matter at the rate of two shillings per pound, the price charged for it at Paris, would cost about six shillings; but the cost of this is more than covered by the twelve pounds and a half of boiled meat. The price of dried gelatin may, as I have stated above, be readily brought down to one shilling and three-pence per pound. In that case, a man's food, seasoning included, would cost three half-pence, and the reduction of the rations of bread and vegetables would occasion a saving at least to that amount.

Atlas Ethnographique du Globe, ou Classification des Peuples Anciens et Modernes d'après leurs Langues; précédé d'un Discours sur l'Utilité et l'Importance de l'Etude des Langues appliquées à plusieurs branches des Connoissances Humaines; d'un Aperçu sur les Moyens Graphiques employés par les différens Peuples de la Terre; d'un Coup-d'œil sur l'Histoire de la Langue Slave et sur la marche progressive de la Civilisation en Russie, avec environ sept-cents Vocabulaires des principaux Idiomes connus; et suivi du Tableau Physique, Moral et Politique des cinq Parties du Monde; dédié à l'Empereur Alexandre, par Adrien Balbi, *Professeur de Géographie, de Physique, et de Mathématiques.* Un vol. in folio, et un vol. in oct°. *Prix* 30 *francs. Chez* Rey *et* Gravier, *quai des Augustins.*

Languages, in the permanency of their characters, their analogies and mutual relation, contain the history of every people: essential guides in the labyrinth of the descent of nations, the study of them is indispensable to all who would investigate the races from whence nations have sprung, or who would undertake their methodical arrangement.

M. Adrien Balbi, already advantageously known by his *Compendio di Giographia universale*, and his *Essai Statistique sur le Royaume de Portugal*, has profited by the researches of the most distinguished philologists to complete a work of which no previous example existed, but of the first importance in a variety of questions which relate to History, Geography, and Philology. Discarding hypothesis, and uninfluenced by system, he has united, under the title of the *Atlas Ethnographique du Globe*, all the well-established facts which the study of languages, alike vast and difficult, had previously made known. With discretion to doubt, where many others would unhesitatingly decide, he rarely if ever conducts to error; and reasoning skilfully, wherever the facts are sufficient to bear him out, his discussions are calculated to place probabilities in the clearest light. Undismayed by the innumerable difficulties, which presented themselves at every step in the execution of his prodigious undertaking, he has classed all the known languages, according to their analogies or differences, in *groupes*, *règnes*, and *familles*; these latter are again subdivided into *langues*, *dialectes*, *sous-dialectes*, *variétés*, and *patois*. The connexion of the principal divisions of Ethnography is traced with the leading

features of History and Geography; and the genealogical roll of the human race, from the earliest times to the present day, is unfolded, with equal conciseness and sagacity.

The work of M. Balbi upon languages may be compared to those of Linneus and Jussieu upon plants, of Cuvier upon animals, and of Brogniart upon mineral substances. To a general classification, it adds a specific description of each language, conceived in few words, but comprehending every point important in discrimination, or interesting in respect to general knowledge.

M. Balbi has rendered a great service to men of letters and to the world in general, in thus collecting and methodically arranging, in a single work, a vast variety of facts, previously scattered in numberless publications and in different languages; many of the publications from whence they are drawn being also rare, and others of great cost. Guided by the advice, and aided by the zealous co-operation of several of the most distinguished authors of France, and of the continents of Europe and America, the subjects on which he treats have frequently the additional value of the sanction of the names of the highest authority in each. We proceed to lay before our readers an outline of the work itself, conceiving that, by such means, we shall best enable them to form their own estimate of its utility and importance.

The *Atlas Ethnographique du Globe* consists of three distinct parts:—1st, The *Introduction*, being one volume in octavo; 2nd, the *Atlas*, one volume in folio; and, 3rd, the *Tableau physique, moral, et politique des cinq parties du monde:*—the third part is still in the press, but is announced for publication in September, and will form a second octavo volume. The first and second parts are alone the subject of the present notice.

The *introduction* contains, first, a preliminary discourse; and, second, a treatise in accompaniment and reference to the Atlas. The preliminary discourse commences with a review of the brilliant results derived in our own æra from the study now so extensively cultivated of the oriental languages,—enumerates the principal living philologists,—and points out the important services, which the societies established in the presidencies of British India have already rendered in Ethnography, History, and Geography; and the still more important services, which, in conjunction with the Asiatic Societies of Paris and London, they may be expected to render. It then enters on an examination of the peculiar province, and of the advantages to be derived, from "*La*

Linguistic," or languages considered in their relation to each other. We shall briefly enumerate a few of the principal advantages which are thus contemplated.

M. Balbi considers language as the principal character by which nations may be distinguished; and as furnishing occasionally the *only* guide to the Historian and the Geographer. The truth of this position it would be difficult to contest, since the relation of languages is the only one which, to a certain point, is unalterable; whilst, considered in their geographical or political relations, nations are subject to continual change. To impress more strongly the importance of this hitherto almost neglected study, M. Balbi instances many serious errors committed by authors of distinguished reputation, which would certainly have been avoided, had due regard been paid to Ethnography as an element essential in their reasonings.

The application of this science is shown to various branches of human knowledge; to ancient and modern geography; to botany, to zoology, and mineralogy; and in tracing the early progress of civilization in different countries. We may instance as applications which M. Balbi has himself made of the science of which he has endeavoured to establish the foundation,—the proofs which he has collected of the great extension of the *Guarami* in America; of the *Celtic*, *Germanic*, and *Basque* nations in Europe; of the Persian origin of the *Boukares*, and of the *Ossettes;* the common descent of the people of *Lapland* and of *Hungary;* and the essential difference between the *Ouigours* and the *Yougours*. By an analysis and comparison of their respective languages, he shows that the *Lettons* have received their civilization from the Germans, and the *Amazigh* from the Arabians; that these last people have had considerable influence on the civilization of the Portuguese and Spaniards; and by an ingenious investigation, of which the facts supporting it are taken from Crawford's *History of the Indian Archipelago*, the race inhabiting the South Sea Islands is shown to have possessed a native centre of civilization. By the evidence also of Language, it is proved that the cat, the dog, and the pig are indigenous in that quarter of the globe; and the native country of a variety of other animals, and of plants, is satisfactorily traced.

Having shown the many useful purposes to which the examination of languages may be applied, M. Balbi guards against the danger of pressing too far reasonings founded on analogy, or of resting too much on Ethnological inferences,

by assigning the proper limits within which the application which he recommends should be restricted. We consider this part of the preliminary discourse as particularly deserving of attention, being treated in a manner altogether new, and illustrated by examples either new or previously very little known.

Remarks on the orthography of proper names, and on the compilation and transcription of vocabularies collected by travellers, conclude the preliminary discourse. These remarks appear to us apt and sensible, and it would seem are much required, since it is amusing to learn the serious, and occasionally the ludicrous, errors into which navigators, even of the first celebrity, have sometimes fallen: thus Bougainville gives the Tahitian word for *night* as signifying *day;* and Labillardière, imposed upon by the islanders of Tonga, has gravely inserted as their numerals, words, of which the signification, had he been aware of it, would have prevented their appearing in his pages.

In the *introduction to the Atlas*, M. Balbi, aided by the advice of MM. Malte-Brun, Pinheiro, and Abèl-Ramusat, has described the method to be pursued in tracing nations, by means of their Ethnographical relations, to their respective sources; and has pointed out the principal breaks and interruptions which as yet present themselves to an attempt to render this history complete. He states the grounds on which he estimates the number of distinct languages at upwards of two thousand; and has comprised within the small space of nine pages an excellent essay on the *Bibliography of Languages.* Such are the contents of the first chapter, which we consider as the most important, both as treating in a very able manner the most difficult questions in Ethnography, and as including two most valuable letters—one from the late M. Malte-Brun, and the other from M. Pinheiro Ferreira—both highly deserving of deep consideration by all who occupy themselves in studies of this nature. These letters establish the bases on which the methodical classification of languages should alone be attempted; and freely discuss the present imperfections of a science which must yet be esteemed in its infancy.

In the second chapter, M. Balbi has described the several modes of writing employed in ancient and in modern times. Assisted in this chapter by MM. Champollion, Abel-Ramusat, Saint Martin, and Depping, he has reduced them to the seven following classes, containing the same number of different systems of writing:—1st. Pictorial representation;

2nd. Symbolic representation; 3rd. The modern Chinese characters; 4th. Mixed characters, or the Hing-Ching of the Chinese; 5th. Syllabic writing, in use among the Japanese; 6th. Alphabetic writing; and 7th. Modes of writing not comprehended in any of the above; such as the Quippos of the Peruvians and of the Mexicans; the Notched-sticks of the Montenegrins, and of the people of the North and Centre of Asia; Arithmetical Cyphers, and Chronograms.

Chapters 3, 4, 5, 6, and 7 contain respectively an exposition of the considerations, by which the author has been guided in his classification of the languages of Asia, Europe, Africa, Polynesia, and America. These chapters contain a vast collection of facts, alike curious and important, forming the basis on which the Tables of the Atlas are founded. In assembling these, the author has been assisted by MM. Abel-Ramusat, Saint Martin, Champollion Figeac, Hermés, Freycinet, Duperrey, Lesson, Gaymond, Blosseville, Humboldt, Gallatin, Malte-Brun, Warden, Dubois, Langlois, &c. In discussing the languages of the South-Sea Islanders, M. Balbi does full justice to the principal share which British navigators have had in collecting the materials of which he has availed himself, without, however, undervaluing the services in the same cause which other nations, particularly the French and Spaniards, have contributed.

The 8th and last chapter presents a summary review of the literature of Russia, from the first period of the political existence of that nation to the present time. Having no necessary connexion with the distinct object of the Atlas, the introduction of this chapter is probably due to the peculiar circumstances of the author in regard to the Emperor Alexander, to whom the work is dedicated. The matter which it contains is, however, so entertaining and instructive, that we can hardly permit ourselves to criticise its insertion.

The *Atlas Ethnographique* consists of 41 tables, of which 36 are Ethnographic, and 5 Polyglott. The 36 Ethnographic Tables present all known languages, both ancient and modern, described and classed according to their respective relations. The 5 Polyglott Tables contain the words, *sun*, *moon*, *day*, *earth*, *water*, *fire*, *father*, *mother*, *eye*, *head*, *nose*, *mouth*, *tongue*, *tooth*, *hand*, *foot*, *one*, *two*, *three*, *four*, *five*, *six*, *seven*, *eight*, *nine*, *ten*, being 26 in number, in about 700 languages and dialects. These two species of tables, which are a check upon each other, exhibit the most complete, the best founded, and the most methodically arranged account of the present state of our knowledge in

respect to languages, which exists. Each particular table contains a description of the languages which are included and classed in it, specifying for each the names of the nations who speak it, the countries they inhabit, and the peculiar characteristics of its grammar and its pronunciation; noticing its principal dialects, sub-dialects, and such variations as are important, either in their historical or ethnographical relations:—finally, the alphabet which is employed, and the principal features of its literature and monuments, supposing it to possess either. We are induced to give the following specimen of the manner in which this part of the work is executed, from a sense of the utter inadequacy of any description of our own to do it justice; and we purposely select his description of our own language, as being that by which our readers generally will be best enabled to form their estimate.

"180. ENGLISH.—Spoken in England, in the south and east of Scotland, in parts of Ireland, and of the principality of Wales; in the principal towns, and by the educated, in the remainder of Scotland, Ireland, and Wales; in the islands of Shetland, Jersey, and Guernsey; by the descendants of the English, and by many others in Asia, Polynesia, Africa, and British America; by the greater part of the citizens of the United States of America. English is also spoken extensively in all nations, in all parts of the world, by reason of its importance in literary, political, and commercial respects; from the two latter causes, especially, is common in Hanover, the Ionian Islands, Malta, Portugal, Brazil, and in the Republic of Haiti. The English language is a mixture of Anglo-Saxon and of Franco-Norman, with some Celtic and more Romanesque words. Very rich and very energetic, it is the most simple and the most monosyllabic of all the languages of Europe, and is that in which the pronunciation differs most from the writing. It has two inflections only for substantives; six or seven for the persons and tenses of verbs: the gender is not distinguished, except where sex exists: the adjectives, participles, and articles, are indeclinable. It did not become the language of the government until the reign of Edward III., since which time it has been fixed, and has gradually improved. From the commencement of the seventeenth century, this beautiful language may be considered to have been methodically developed, and from the commencement of the eighteenth, to have acquired its fixed and invariable forms. In literature it occupies a place amongst the most eminent in Europe, ceding to none in polish, and being the first in energy. It admits conciseness, without prejudice to the grace of expression; it is peculiarly suited to the description of the magnificent or sublime in nature. In political and parliamentary eloquence it is without

rival. The English literature, like many others, commenced in the twelfth century, by translations and chronicles, and reached its greatest splendour in the seventeenth and eighteenth centuries. Rich as varied, it still rivals the literature of the most celebrated of the Continent. Its most ancient records are, a Hymn to the Virgin, by Godric, who died in 1170; the translation of the Romance of Brut de Wace, by Layamon or Lazamon; the *Castel of Love* of Robert Grosthead, of the first half of the thirteenth century; and the Chronicle of Robert Gloucester, in the second half of the same century; the works of Robert Brunne, Chaucer, Adon, John Gower, and Robert Langeland, author of the celebrated satire called the Visions of Piers Ploughman, all of the fourteenth century. It appears to us, that the four following dialects may be distinguished in the English, subdivided into several sub-dialects and varieties: first, the ENGLISH, properly so called, which, polished by Chaucer in the fourteenth century, became the written and general language of the nation; its sub-dialects are that of the *City of London* (the Cockney) (*quære*), of *Oxford*, of *Somerset*, the English of *Wales*, and the English of *Ireland;* the *Jowring*, spoken in Berkshire, and the rustic of *Suffolk and Norfolk*. 2nd. The NORTHUMBRIAN ENGLISH, which might also be called the DANO-ENGLISH, in consideration of the great number of Danish words which it preserves; which has, as sub-dialects, the *Yorkshire*, the *Lancashire*, the *Cumberland*, and the *Westmoreland*. 3d. The SCOTCH, or the ANGLO-SCANDINAVIAN, in which are distinguishable the *Lowland Scotch*, formerly spoken at the court of the kings of Scotland, in which James V. composed poems of considerable beauty, Allan Ramsay a pastoral, which by its simplicity and elegance brings to the recollection the Aminta of Tasso, and which Burns has still more recently adorned: the *border language*, a mixed idiom, spoken on the frontier provinces of Southern Scotland remarkable by its Ballads; and the Orkney, which is much mixed with Norwegian. 4th. The ULTRA-EUROPEAN ENGLISH, spoken in the British colonies and the United States of America, and the most prevalent language, generally, in the two Americas."

In the thirty-six tables, the author has distinguished and classed 860 languages, and about 5000 dialects: in addition to this prodigious number methodically arranged in the Atlas, Mr. Balbi names in different chapters of the introduction almost as many more, which, from the want of vocabularies, he has not permitted himself to include in the tables. The General Ethnographical Map of the World, with which the Atlas commences, contains much curious and important matter: it is there seen, that in the present state of our knowledge, the Asiatic languages amount to 153; the European, to 53; the African, to 114; the Polynesian, to 117; and the American, to 423.

Each of the tables is preceded by an introduction, in which

are traced the geographical limits in which the languages which are classed therein are spoken ; and a rapid sketch is given of the physical geography, the history, the manners and the moral condition of the people comprehended in the ethnographical region under notice. It is, we believe, the first attempt of the kind to view the people of the globe in regard to their divisions by language, and presents, as it could not fail to do, many new and remarkable views, recommended by general elegance of style, and occasionally enlivened by vivid and brilliant representations. We might cite, as examples, the introduction to the languages of India, to the Basque and Celtic *families;* the sketches of the Greco-Latin, Germanic, and Slavonic nations ; and those which precede the classification of the languages of the Orinoco-Amazon, the Alleghani, and of the Great Lakes. We had marked for insertion, the introduction to the family of Germanic languages, but we approach the limits of our space, and can only, therefore, refer such of our readers as may have access to the work to the commencement of the 13th Table, as a good specimen of the manner in which this portion of the work is executed.

Our duty as critics obliges us to remark that there are some parts of M. Balbi's classification of languages in which we cannot fully coincide. His general division of the Germanic languages, for example, differs from the system followed by the German writers themselves ; a system justified by the analogies existing in the different idioms comprised in that family. The *Gothic of Ulphilas*, or the *Mesogothic*, appears to us to have more analogy with the ancient high German, than with the three modern idioms of Scandinavia, the Norwegian, the Danish and the Swedish, and should be classed accordingly. Being, moreover, the most ancient of the German languages, it is the more important that the example of the German philologists should be followed, who all agree in placing it at the head. It is true that M. Balbi himself is not responsible for this classification, having followed, in this particular, the opinion of the late M. Malte-Brun, by whom he was specially directed in regard tothe languages of that group. Writing at Paris, he could not have there found a better guide ; and we should willingly concede our difference of opinion, could he give to that classification, to which we object, the support of the evidence which M. Malte-Brun proposed to bring forward, in respect to the people of Scandinavia. In the present state of the question, however, and until M. Rask shall make known those arguments which may be urged on the behalf of the

partisans of *Scandinavianism*, M. Balbi must permit us to retain the opinion which appears to us best founded, and which is also that of the Germans themselves.

The Dutch language is also, we think, treated too lightly, considering its great literary importance in the seventeenth century. Instead of the Metrical Chronicle of Nicolas Colyn, now no longer doubted to have been composed in the seventeenth century, the author of the Atlas might have cited the extensive historical collection of Jacob van Maerlant, known under the title of the *Spegel Historiael ;* the *battle of Woringen* of Jan von Helm, an historical poem of great merit ; and some others, of sufficient importance to have required notice, in following, in respect to the language of the Netherlands, the plan adopted in the other languages. Here, also, we must again remark, that our criticism does not apply so much to the author of the Atlas, as to the learned philologist by whom the article relative to that particular language was supplied.

Other critical remarks, of the same nature, have occurred to us: we might also point out omissions ; some inadvertencies ; some opinions ventured, as it appears to us, without sufficient ground ; fewer, indeed, in the work of the author himself, than in the articles supplied by the many persons by whom he has been assisted : but we abstain; because, having carefully examined, we are assured that the faults are few in number, of very minor importance, and that several find their correction in the work itself. We must not omit to praise the typographical accuracy, which is the more meritorious, considering the immense number of foreign words which almost every page of the text contains, and which, in the Tables, amount to upwards of 16,000.

The *Tableau physique, moral et politique des cinq parties du monde*, which is in the press, and of which we have seen the manuscript, compresses in fewer pages than we had thought it possible, the principal details of physical and political geography arranged and methodised.

We shall conclude this article by observing, that the *Atlas Ethnographique du Globe*, by the extent of its plan, and the distinguished names which have co-operated in its execution, must be regarded as a classic work in the science of languages ; as the most natural introduction to the study of geography and general history ; and as deserving a place by the side of the *Art de vérifier les Dates*, and the *Tableaux historiques de l'Asie* of M. Klaproth.

Hieroglyphical Fragments.

[By a Correspondent.]

i. *Corrected* Translation *of* Mr. Grey's *Enchorial Manuscripts.* Hieroglyphics, 34, 35. Young's Account, p. 76.

A.

(1.) The year XXVIII; in the reign of Ptolemy and Cleopatra his sister, son and daughter of Ptolemy and Cleopatra, the gods Illustrious: (2) and the priest of Alexander, the Saviour gods, the Fraternal gods, the Beneficent gods, the Father loving gods, the Illustrious gods, (3) and the Mother loving gods: the prize bearer of Berenice the Beneficent, and the basket bearer of (4) Arsinoe the Brother loving, and the priest of Arsinoe the Father loving, being [as by law appointed] in the metropolis [Racot or Alexandria], and in the royal city (5) [Ptolemais of the Thebaid], the Guardian Priest of Ptolemy Soter, and the priest of Ptolemy the mother loving, and the priest of (6) Ptolemy the brother loving, and the priest of Ptolemy the Beneficent, and the priest of Ptolemy the Father loving, (7) and the priest of Ptolemy, the god Illustrious and Munificent, and the priest of the queen Cleopatra, and the priest of the Cleopatra, the king's (8) 'mother? the goddess Illustrious, and the basket bearer of Arsinoe the Brother loving, being [all as by law appointed]. It is declared that the parties (9) Alecis the son of Erieus, and Lubais the daughter of Erieus, and Tbaeais the daughter of Erieus, all children of one mother [Thautis?], Senerieus the daughter of Petenephotes, (10) and of Senlubais, Erieus the son of Amenothes, and Senosorphibis the daughter of Amenothes, both daughters of Senamunis, Sisois [or Spois], surnamed (11) Erieus, the son of Amenothes and Senchonsis; all seven [living] in Diospolis the splendid; to the parties Teephibis the son of (12) Amenothes and 'Mautis? have sold for a sum of money seven and a half rods of unproductive ground. (13) These seven and a half rods lying in the southern part of the whole ten rods of open ground in the 'south-west of the city? the remaining (14) two and a half rods having been purchased by Asos the son of Horus and of Senpoeris, 'as a tenement for

himself and his family? in the XXVIIIth year of the king (15) ever living. The ten rods of ground have to the south ‘ the inhabitant Haines? The seven and a half rods have to the south-‘west (16) Emiamun the scribe, with his family, and his brother with his family? The two and a half rods of Asos, the son of Horus and of Senpoeris, (17) ‘ have to the east the Course of Juno? “ to the west the Royal road leading to the temple of Ceres?? For the seven and a half rods of ground ‘ with their buildings and all their appurtenances? to wit, Seven and a half rods of ground: The price in money was paid and received, (19) that is to say, ‘ three? talents (19, 20, 21). ‘ And the contracting parties agree that the sellers shall make no further demand on the purchaser, and shall defend him from any disturbance therein, or indemnify him in case of their failure so to do: and shall guarantee the validity of the sale? (21) ‘ Executed in the month Pachon? transacted in due form ...(22)..(23)..(24).. Written by Erieus the son of Phanres, clerk to the chief priests of Amonrasonther and the contemplar gods.. Amen.

Greek Registry.

In the year XXVIII; the 28 Mesore [XII]. Transacted at the table in Hermopolis [or Hermonthis], at which Dio. [nysius] is the collector of the circular twentieth, in the account of Asclepiades [contractor for the tax on sales], of which the subscribing clerk is Ptolemais the countersigner. The purchaser being Teephibis the son of Amenothes: for seven and a half rods, towards the south, of the whole unproductive ground which is in the southern part of Diospolis the Great; of which the boundaries are given in the annexed agreement; and which he bought of Alecis the son of Erieus, and Lubais and Tbaeais the daughters of Erieus, and Senerieus the daughter of Petenephotes, and Erieus the son of Amenothes, and Senosorphibis the daughter of Amenothes, and Sisois [or Spois] surnamed also Erieus, the son of Amenothes. The XXVIIIth year; Pachon 20. ‘ III? talents. On which the duty is 900 drachmæ. 900.

Dionysius, collector.

B.

(1) The year XXVIII; in the reign of Ptolemy and Cleopatra his sister, (2) the son and daughter of Ptolemy and Cleopatra, the Illustrious gods; (2) ‘ like to the sun ? ever-living. (3) It is declared that the parties, Alecis the son of Erieus, and Lubais the daughter of Erieus, (4) and Tbaeais the daughter of Erieus, the three by the same mother, ‘ Tasuis ? and Senerieus the daughter (5) of Petenephotes, and of ‘ Sen. ?-Lubais, Erieus the son of Amenothes, and Senosorphibis (6) the daughter of Amenothes, both by the same mother Senamunis, Sisois, surnamed Erieus, the son of (7) Amenothes and of ‘ Senchonsis ? the seven being mechanics in Diospolis (8) the Splendid; to the party Asos the son of Horus and of Sempoeris; have sold (9) for a sum of money two and a half rods of unproductive ground: these two and a half rods being (10) situated to the north of the whole ten rods lying in the ‘ south-west quarter of Diospolis ? (11) the remaining seven and a half rods having been purchased by Teephibis the son of Amenothes and (12) of ‘ Mautis ? towards the south, as a tenement for himself and his family, in the XXVIIIth year (13) of the king everliving: the ten rods having to the south the ‘ inhabitant (14) Haines? the two and a half rods having to the south the seven and a half rods of Teephibis the son of (15) Amenothes and ‘ Mautis ? “ to the east the Course of Juno ?? to the north the house of (16) Psenchonsis ? the son of Spotus ?? and ... “ to the west the Royal road (17) leading to the temple of Ceres ?? For these two and a half rods of ground (18) not cultivated; that is to say, for two and a half rods of level ground (19) the price was paid and received, that is to say, ‘ five ? talents. (20 .. 28) [As in A, 19 .. 24.]

Greek Registry.

In the year XXIX; Phamenoth [IX.] 9. Registered at the table in Hermopolis, at which Dionysius is collector of the tax of five per cent., in the account of Asclepiades and Crates [contractors for the duty on sales], of which Ptolemais the countersigner subscribes: Asus the son of Horus, the purchaser of two and a half rods of unproductive ground,

lying in the southern part of Diospolis the Great, of which the boundaries are given by the annexed deed, which he bought of Alecis the son of Erieus, and Lubais and Tbaeais the daughter of Erieus, and Senerieus the daughter of Petenephotes, and Erieus the son of Amenothes, and Senosorphibis the daughters of Amenothes, and Spois, surnamed Erieus, the son of Amenothes, for five talents, on which the duty is 1500 drachmæ. Dionysius, collector.

C.

(1) The year XXXV; in the reign of Ptolemy and Cleopatra his sister, the son and daughter of Ptolemy and (2) Cleopatra, the Illustrious gods: and the priest of Alexander the Saviour gods, the Fraternal gods, the Beneficent (3) gods, the Father loving gods, the god Defender of his father, and the Mother loving gods; and the prize bearer of (4) Berenice the Beneficent, and the basket bearer of Arsinoe, the Brother loving, and the priest of Arsinoe, the "Brother" 'and? (5) Father loving, being all [as by law appointed] in the metropolis. It is declared, that the men in 'the service of Amun? of the family of 'Philochemus? (6) Ammonius the son of Pyrrhius.. Zbenaetes Psenamunis the son of Pyrrhius, both sons of 'Theothis? and both mechanics in (7) Diospolis the Splendid, to the party Pechytes the son of Arsiesis and 'Thenune? have sold for a sum of money one fourth part (8) of the whole $13\frac{1}{3}$ rods, 'that is to say, [α] $3\frac{1}{3}$ rods, being of the $13\frac{1}{3}$ rods the fourth part? The $3\frac{1}{3}$ rods (9) [α] $33\frac{1}{3}$? rods. The three and $\frac{1}{3}$ rods are bounded by [the properties of] Teephibis the son of Amenothes, and Asos the son of Horus, both 'Cholchytæ? making together (10) ten rods, bought by them, and occupied by their 'brotherhood? the whole being bounded on "the south by the inhabitant Pamonthes, surnamed (11) Petechonsis, and Chasytes the son of?? Petenephotes.. 'to the south-west the house of Petechonsis?.. (12) 'and the house of Philinus?.. 'the late? Erieus... (13) the house of Arsiesis... 'Sentathlyt? 'and her children?.. the house of Psenchonsis.. (14) 'the Royal road? 'leading to the temple of Ceres? "and to the river near the temple?? 'the house of Haines? 'All these are the boundaries of? (15) the $13\frac{1}{3}$ rods of ground, 'uncul-

tivated, and within the city? 'occupied by a house? $13\frac{1}{3}$ rods (16) 'within the city?? $3\frac{1}{3}$ rods of ground [α] $33\frac{1}{3}$? $3\frac{1}{3}$ rods of ground not cultivated (17), the price was paid and received in silver, II talents ... (18..22) (Nearly as in A and B). (22) Written by "Pathecis the son of Thalubis?? clerk to the chief priests of Amonrasonther, and the contemplar gods, (23) the 'Beneficent? gods, the Father loving gods, the Illustrious gods, the god Defender of his father, and the Mother loving gods. Amen.

Greek Registry.

In the year XXXV; Pharmuthi (VIII) 20. Registered at the Table in Diospolis the Great, at which Lysim[achus] is collector; in the account of Sarapion and his partners, [contractors for the duties] on sales, for which the subscribing clerks are Hermophilus and Sar[apion] the countersigners: the purchaser being Pechytes the son of Arsiesis: one fourth part of a piece of uncultivated ground, $3\frac{1}{3}$ rods, in the southern part of Diospolis the Great, to the west of the Course of Juno, leading to the river, of which the boundaries are shown by the annexed deed; which he bought of Ammonius the son of Pyrrhius, and Psenamunis the son of Pyrrhius. Two talents of brass, the duty on which is 600 drachmæ; 600. Lysimachus, collector.

ii. *Part of the Registry of* Mr. Salt's *Papyrus. Hier.* 36.

(1) In the year XLVII; Tybi [v] 5. Registered at the table in Hermapolis, at which Dionysius is collector of the circular Twentieth, in the account of 'Hermias? for which (2) Isidorus is the subscribing clerk ... rods 27 | 35.. A house, being part of the property belonging to 'them? ... [bought of] (3) 'Anepies? son of Ach .. and 'Spoites? son of Teephibis. Eight talents; the duty "drachmi" two thousand 400. Dionysius. 'Apollonius?.

iii. *Note on Plate* 49. The Enchorial name over the suppliant is Bernice, written as usual from right to left, though the figure looks to the left. This is a singular confirmation of the remark in p. 43 of the last number of this Journal, with respect to the Enchorial character, although the "question" is there made too general as relating to writing

with the pen, since there are numberless tablets so written, in which the figures and the corresponding hieroglyphics stand in both directions: but the words were used with reference to writing only, without pictorial figures.

The same word is repeated at the foot of the tablet: and it appears in the first line of the Hieroglyphics nearly as exhibited in the Article Egypt, except that a T is improperly inserted for an I. In the second line we have ARSNE. The T seems here also out of place: but as a generic termination it seems not to be always phonetic. The suppliant has not the usual dress of a female, though without a beard.

iv. *Plate* 51. Line 1. R . . O. This word is CLEOPATRA, though much defaced and not quite correctly copied: the *upright* line for R is however right. The name is repeated at Es, but the R is here inclined as usual. At Z s we have PTOLEMY, and at Xs..t, though partly effaced. It is followed by the word *surnamed.*

v. *Tablets from Sachara, yet unpublished.*

In the Egyptian room at the British Museum there are a number of chalk tablets, from the mummy pits at Sachara, sent expressly by Mr. Salt, for assisting in the comparison of the sacred characters with the Enchorial. Some of them will probably appear in the Collection of Hieroglyphics, containing the articles already mentioned. Among the most remarkable of these are two which appear to be companions, the one having 7 stars in the margin at the summit, the other 19. The Enchorial dates appear to be 6 and 19 respectively: showing clearly that these *stars* must have been meant for *years.* The names of CLEOPATRA and AUTOCRATOR CESRS occur repeatedly in both: affording a striking confirmation of Champollion's hieroglyphical expression of these same epithets. They have not before been recognised in the Enchorial form. Hier. 70 . . 74.

There is another large tablet of Enchorial characters, bearing the name of PTOLEMY SURNAMED CÆSAR, and not Neocaesar, as Champollion has read the surname in another form. Hier. 75, 76.

On a broken and somewhat imperfect tablet of chalk, brought also from Sachara, are two Royal names, which

appear to be ARSNE, or Arsinoe, and PHILPATRA, for Philopator; the queen, whose priestess is mentioned in the inscription of Rosetta. There is a coarse though small Enchorial inscription, with three nearly consecutive names of PTOLEMY, and with the word SCHR or SOCHAR, probably the Socharis Paamyles of Cratinus in Hesychius, as Champollion reads it, calling it an epithet of Phthah. The characters are here rather Hieratic than Enchorial; and it becomes not at all improbable, comparing this inscription with some others, that the name of Saccara is derived from the deity supposed to preside there, and that the name should be written *Sachara*, or Sacchara. In the portfolio of Mr. Salt's Papyrus there is one, in a sort of Hieratic character, with some pictorial repetitions, which has the word CANDACE on the back in Greek characters.

vi. *Alphabet of the Nail headed Characters.*

The recovery of the lost literature of Egypt appears to be of so much the more importance, as it has opened a prospect of affording a key to the characters of the ancient Persians. Our hopes of success in the research must, however, in some degree be depressed, when we find that the key has already been for some time in existence, and has not served to procure us access to anything very interesting, at least while in the hands of the first discoverer.

There is, in the library of the East India Company, a manuscript page, apparently intended for engraving, which was brought or received from the continent by the late John Walker, Esq., F.R.S., and by him presented to Mr. Wilkins. It contains a Persepolitan alphabet, compared with Russian, Persian, and Roman letters; and it is remarkable that by this alphabet almost every letter in the name of Xerxes, as identified by Mr. Champollion and Mr. St. Martin, is read in the same manner, except the initial aspirate, which the anonymous author makes an O: while on the other hand it is singular that the corresponding hieroglyphic on the vase was also read by Dr. Young as an O. There is, however, another exception in the substitution of E for B: that of R for L is rather a confirmation of Mr. Champollion's reading, than an exception.

Mr. St. Martin's alphabet has been published in the Journal Asiatique for February, 1823; he says it agrees in many respects with that of Grotefend, and it is here compared with the alphabet of Mr. Walker's correspondent.

Persepolitan.	St. Martin.	Russian Author.
	KH	Ô
	SH	Σ
	E	B
	A	â
	Å	â
	R	Λ
	H	N,Σ
		omitted
	Y	ă,(M)
	I	ê
	E	ă
	E	ĕ
	B	B
	D	M
	OU	Y
	OU	ü
	P	ă
	N	ä
	V	ä
	T	ü, "½ ü"
	S	T.

In the inscription, the thicker end of the wedge is always uppermost, or to the left; it is here expressed by a simple line.

Mr. Wilkinson has lately found a hieroglyphical name in Egypt, which he reads *Artaxerxes.*

On Achromatic Microscopes, with a description of certain Objects for trying their Defining and Penetrating Power.

By C. R. Goring, M. D.

If my readers should be at a loss to comprehend some of the allusions in this paper they will have the goodness to refer to a treatise of mine, in the last number of the old series of this Journal, "On thick Aplanatic Object-glasses for diverging Rays, &c."* I hope I shall be pardoned if I digress slightly from my present subject to notice the praiseworthy exertions of Mr. Dollond, who has been stimulated by my agitation of the subject of aplanatic object-glasses for microscopes, to apply his vast resources to their construction. His success has been such as might be expected from a descendant of the great father of achromatic lenses. He has, *without effort or difficulty, and with the same precision and certainty which he has attained to in the manufacture of the larger glasses*, produced three triple small ones, of somewhat less than an inch focus. One is in the possession of the celebrated Baer; it has an aperture of about 0·4. When I say that it has given the highest satisfaction to such a judge, every other encomium must be superfluous. The two others I have seen; they have a naked aperture of 0·45 (for Mr. D. disdains to cut any of it off): the convexes are both of crown glass. I can scarcely venture to say that they are perfect on *opaque* objects with the entierty of their diameter; but a reduction of 0·05 would, I apprehend, make them so. These glasses are burnished fast into their cells in the most rigid adjustment: this is a great desideratum. Mr. Tulley's are only screwed together, and are in consequence deranged by the slightest concussion. Mr. D. has combined his two object-glasses nearly in contact. It will not be credited, perhaps, without ocular demonstration, that in spite of the contraction of the focus by this operation, the totality of the aperture of 0·45 is still as good or better than with

* I had originally intended the present essay to form a continuation of the subject; but it may, with more propriety, be resolved into a distinct tract.

either glass singly. There is, however, an inevitable dullness produced in the image of an opaque body, by the multiplicity of the surfaces of the six lenses, somewhat similar to what occurs when an ordinary object-glass is dimmed by moisture; with transparent objects, this defect is scarcely felt. I need not say that an object-glass of such power, so divested of both kinds of aberration, and possessing so large an angle of aperture, must be a match for any test object whatever; accordingly, every difficulty at once yields to it, overwhelming efficacy: notwithstanding, I should, myself, prefer a single triple-glass of the same depth as the combination, at least for opaque objects, which are all within reach of such an angle of aperture as it would be capable of receiving—and such an objective it isin Mr. D.'s contemplation to make.

I have, in my paper on aplanatic glasses already alluded to, said much of the extreme difficulty which Mr. Tulley experienced in perfecting his object-glasses, which was wonderfully increased with the reduction of their foci. It will be recollected that his first essay was upon one of nearly as short a focus as 0·3; had he commenced with a longer ones doubtless he would have accomplished his purpose far more easily. The chief difficulty he seems to have had to contend with, was the perpetual tendency of his tools (though kept in pairs with steel guages) to alter their figure by some quantity inappreciable by the said guages, yet enough to mar the performance of the object-glass when worked. Mr. D.'s pre-eminent tact and management have overcome this obstacle, which will of course enable him to work to a regular theory. Whatever may be the merit of overcoming difficulties, that of precluding or exploding them altogether is still greater; and Mr. Tulley will, I am sure, consider it no disparagement to his own talents to admit, that as Mr. Dollond can work the small aplanatics with certainty and dispatch (which Mr. T. confessedly cannot), the said Mr. D. is the better man, and more consummate artist, by whom it is no disgrace to be beaten.

I here commence the proper subject of this paper.

Length of the body.—It seems adviseable to say some-

what concerning the proper length of the body of an achromatic microscope ; but I am afraid it will be impossible to determine the problem with any positive accuracy. From experiment it would appear, then, that the length of the anterior focus of the object-glass should bear some certain ratio to the other conjugate focus, in order that the instrument may do its utmost. Thus, an object-glass of long focus requires a longer body than one of shorter focus ; but it is almost impossible to lay down the precise dimensions which answer or ought to answer best: we may, perhaps, reasonably assume the maxim, that the object-glass will work best along with that body which was annexed to it at its formation, and along with which it was originally made to correct. It may be asserted, I think, that an aplanatic of 0·9 focus will not act well with a body of less length than 9 or 10 inches ; while one of 0·2 focus seems to do its best, or at least do perfectly well, with one of about 6 inches or even less. The tubes may be increased in length beyond this mark, however, without making any very sensible or decided alteration in performance. We might expect, that as the radiant-point always approaches nearer to the object-glass in proportion to the elongation of its anterior focus, we should gain somewhat by the slight increase in the angle of aperture which is thus occasioned, while opaque objects would be seen better, because their light being radiant and not of the intercepted kind, is diminished or increased in proportion to the square of their distance from the object-glass. But it does not seem to me that the human eye is at all able to appreciate the difference in the performance, if any. On the other hand, long tubes are very inconvenient ; they place the eye of the observer at a very disagreeable distance from the stage of the instrument, which can thus hardly be used with comfort for dissection in a vertical position : they moreover require steadying-rods and other contrivances to give them the requisite stability. It may be suspected, moreover, that their rays in travelling over a large space (if they have any error in them) will become still more erroneous, and form a worse image than they would if cut short at a less length. It is well known, that Sir I. Newton invented his telescope,

because he thought that, as in the Gregorian construction, the rays must necessarily pass over a considerable space from the small metal before they reach the eye-piece: the slightest errors would become wonderfully aggravated in this long compound-reflecting microscope (for such it is in its operation on the image of the large speculum). In Sir I. Newton's construction the eye-piece is as close as possible to the small metal; and it is this circumstance which, in my opinion, constitutes the superiority of the Newtonian over the Gregorian construction, which is, I believe, pretty generally admitted.

Now, this analogy will apply to achromatic microscopes, though the errors of rays by refraction are very small in proportion to those by reflection; but on theoretical considerations I am not disposed to lay much weight, unless they are confirmed by practice.

Having thus stated the *pour* and *contre* of the question, I leave every man to the exercise of his own private judgment. I am not such a despot as to expect people to use long or short tubes (when either will do very well) just to chime in with my fancies on the subject, any more than I should wish them to break their eggs at the large or small end, according to my own ideas of convenience. The case seems to me to resolve itself into a matter of opinion. The only real advantage I can discover in a long tube, is to procure a very extensive scale with a shallow eye-piece for micrometrical measurements, which it certainly accomplishes.

Eye-pieces.—Those used with the aplanatic object-glasses are of the negative Huygenian kind, very similar to those employed in telescopes, consisting of two plane convex lenses only. It is well known that these can be rendered perfectly achromatic; and their spherical aberration being incorporated with that which is corrected by the concave lens of the object-glass, the whole of the instrument is perfect though the constituent parts are not. It is not thought adviseable to extend the number of glasses beyond two, for the same reasons which forbid the use of a third or fourth glass in telescopes. If, however, people are to be found so besotted with large fields of view as to prefer them to all the valuable

qualities which are incompatible with them, let it be known that they have only to apply the ordinary quadruple and quintuple eye-pieces to the achromatic object-glass, and they will obtain the number of degrees of extension which they want, (for the size of a field of view, as well as the flatness of it, depends almost entirely on the nature of the eye-piece employed,) at the moderate expense of ruining the brilliancy and sharpness of the image, and introducing such a flare and reflection from the numerous surfaces, as will, under certain circumstances, give an image or two more than are wanted, with a bright luminous spot in the centre of the field!

There is a defect in the double achromatic eye-pieces for microscopes which cannot well be helped: it is, that when glazed in such a manner as to be truly achromatic (in which case the field-glass will be of longer focus and farther removed from the eye-glass than in the telescopic ones), a certain distortion, or convex, barrel-shaped curvature is seen at the edges of the field of view: if, again, this defect is remedied by a different mode of glazing, they will be no longer achromatic; so there is only a choice of evils.

Though a common erecting eye-piece does well enough for the small aplanatics when its third and fourth glasses are placed at the same distance from the object-glass as the other inverting eye-pieces are; yet, if it is thrust in nearer to the object end to lower the power (which it is frequently convenient to do), a great deal of colour becomes visible, at least if the stop between the erecting-glasses is sufficiently enlarged to allow the whole pencil to pass through:—under these circumstances the necessity of introducing a secondary achromatic becomes evident; but, in order to make a workmanlike job of the thing, it would seem to require to be made in two pieces, viz. the fourth a compound of a convex and concave, but so much over-corrected as to neutralize the aberration of another convex at some distance, standing in lieu of the third in the common erecting eye-pieces. Mr. W. Tulley has, I believe, satisfied himself that this species of *triple achromatic in two parts* may be executed, though he has not yet suc-

ceeded in making one exactly to his mind: this will be a new curiosity we shall owe to his genius; the theory of it will serve to amuse the learned.

Mechanical Arrangements.—The merit due to the contrivance of these I hold to be but trifling; I therefore am not very ambitious to constitute myself a Sir Oracle concerning them, and forbid every dog to bark when I speak on the subject; still less do I conceive myself justified in trespassing on the pages of this valuable journal by entering into a detail of my own particular whims about stands and apparatus, which every man will eternally alter, according to his own ideas of utility and convenience. However, there are, I think, certain principles which ought to be attended to, whatever may be the manner in which they are carried into effect. My own experience leads me to recommend the following punctilios to be observed in making a stand, &c.

Thus, 1st, the bar and arm ought to be triangular, because, when pinched by a spring or other force on *one* side only, they are sure to be locked fast in every other direction; which is not the case with square, flat, or round bars travelling in corresponding sockets.

2nd. The stage should be a fixture, both that it may not give way during dissections, and that animalcules and aquatic insects may not be disturbed by adjustments of any kind giving motion to it during observation; for when once unsettled and lost from the field of view, it is often exceedingly difficult, and sometimes impossible to find them again. The body, therefore, or single magnifier, should be able to travel completely over the stage, and possess another motion independent of it for adjusting the focus.

3rd. The stage, in all other respects fixed, should be constructed to swing round on a pivot, with a clamp, that objects placed upon it may be viewed laterally as well as vertically, and be made to remove altogether when occasion may require, to allow of the introduction of large bodies, such as an anatomical preparation, a jar of polypi, specimens of minerals and the like, to the operation of the microscope; for which purpose, it will be convenient that the bar should be attached to the pillar of the microscope, in such a manner

as to allow it (by means of a ball and socket or other similar arrangement) to fold down on one side, and leave the space underneath open, as well as to permit the flame of a candle to be applied immediately behind the stage for transparent objects, or before it for opaque ones. This simple kind of illumination, unassisted by any condensing lenses or mirrors, is by far the most effectual, and the least liable to cause optical deception of any I am acquainted with. As, however, the object-glass and opaque objects are both frequently placed in jeopardy by this arrangement, it will be necessary to employ a condensing lens occasionally, which had better be attached to the body, that its spectrum may always travel along with it. The condenser should be composed of two glasses, of a large angle of aperture, and of the same focus, to be screwed together at pleasure when candle-light is used (which requires their focus to be only one-half of what it is for day-light to give the maximum of illumination); for the brightness of the spectrum depends upon the aperture the condenser has relative to its acting focus.

4th. It will be found very useful to have a kind of boot to slide over the object end of the compound microscope, to enable us to insert it directly into liquids of all kinds to some depth, and examine objects contained therein without disturbing them: a tube, somewhat conical at one extremity, and having a piece of very thin and very accurately-figured plate-glass burnished into it so as to be water-tight, will be a very effective piece of apparatus for this end. Another variety should also be constructed with a diagonal reflector, to give a lateral and periscopic view of the contents of a vessel.

5th. The illuminating mirror should be oval* (if not of very large dimensions), so that it may form a round image in the visual pencil, equal in diameter to it when placed at an

* Having a plain glass on one side, and a face of plaster of Paris at the other, this latter surface, employed to reflect the rays of the sun, gives a beautiful imitation of the light of a whole cloud, so much esteemed by all microscophs for transparent objects. I do not approve of concave mirrors; a bull's-eye lens under the stage does their work better, in my opinion.

angle of 45°: it should revolve at the bottom instead of at the side, as in the common way, that the axis of the ellipse may always present itself in the proper direction: by this arrangement, also, it evidently becomes more manageable.

As all transparent objects are shown by intercepted light furnished by the said mirror, if it is not of due size or proper shape the pencil at the eye-piece will be mutilated, much in the same manner as if the aperture of the object-glass or metal was partly closed. I conceive every stand to be vicious and defective which does not possess the properties I have enumerated; but none of those now made answer completely to all these indications. There is a very superior kind of Ellis's aquatic single microscope, made by Banks in the Strand, and said to be used and recommended by the great Bauer, which approaches nearer to my ideas of a perfect stand than any thing else I have seen; indeed, the authority of Bauer must be allowed to be paramount in all matters relative to microscopes. I have assumed this as a foundation to work upon, and have endeavoured to remedy what I conceive to be its defects and disabilities, and have adapted it to carry an achromatic body; an Amician reflector, or single magnifier, constituting what I should be disposed to call a *regular working tool*, either for dissection, drawing, or observation, which I am pretty confident will be found available on every emergency, and for every purpose to which a microscope can be applied. Should any one coincide with me in my views, as here expressed, and wish to have such a stand as I should myself like to use, he may procure it by applying to T. Cuthbert, 22, Bishop's Walk, near the Palace, Lambeth. This artist is now completing an instrument such as I have described; the patterns for the casts I made with my own hands, under his direction, and assisted by his advice, in order that every thing may be accommodated exactly to my own peculiar conceits on the subject.

I trust the time will come when a set of Amician metals, of achromatic object-glasses, and of diamond single lenses, will be all adapted to the stand and apparatus in question (for it is contrived to answer equally well for the whole triad); and that thus the separate capacities of each of these gigantic im-

provements on the microscope (which must surely form a new epoch in the history of that instrument) may be made to harmonize and blend together in one omnipotent whole, wherein the defects and incapacity of each, for particular purposes, may be borne out and neutralized by the peculiar power and distinct operation of the other component parts; for I conceive it impossible to find any microscopic objects, capable of being developed to human vision, which one or other of the three microscopes will not master and exhibit in the utmost possible perfection.

Test Objects.

Without test objects it is impossible to ascertain whether a microscope is *effective* or not: few opticians have any other ideas concerning the goodness of microscopes than that they must have a *large flat* field of view; presuming that in proportion to the width of the field-bar and the flatness of the image at the margin of it, must the perfection of the instrument be estimated, without any regard I suppose to light, distinctness or achromatism! Nothing can be more absurd and preposterous than this notion, with which, however, I was at one time of day as much imbued myself as any worthy *shoptician* or *glazier* can be at this moment. It is just as ridiculous to estimate the goodness of a telescope in the same way.

I willingly admit that a large flat field of view is a valuable property in either instrument; but, after all, if we can see nothing which ought to be seen in it, it is of no more value than a large piece of ground which will produce nothing but weeds and thistles. The prevalence of these false ideas must be attributed to the want of the knowledge of proper objects to try the goodness of microscopes in the same way as that of telescopes. I believe I must appropriate to myself the honour of having first discovered such, of which I now propose to give some account. A test object, then, may be defined to be *some object which can only be seen by a microscope single or compound, which possesses a great quantity of good distinct light (the result of a large and perfect aperture)*, and which in consequence will serve as a

standard of comparison, whereby to scrutinize the pretensions, the defects, and excellencies of different microscopes, and prove beyond dispute whether they are effective or not*. They will serve as a capital subject of study for those who suppose the goodness of a microscope to depend upon the width and flatness of the field of view.

I here premise a circumstance which must be rigidly attended to, in order to give an instrument fair play: it is that the power of any two microscopes, tried against each other, should be equalized and obtained in the same way; for the better an instrument is, the lower will be the power necessary to give it the ability to demonstrate a difficult object. That microscope, therefore, which will show given objects with the lowest power is most certainly the best, be it what it may. The analogy holds good with regard to telescopes also; for I have ever remarked, that all the perfect instruments of this kind which I have seen were distinguished by their power of showing difficult double-stars, &c., with very low charges. Nothing exasperates me more than to hear some half-taught booby bragging of the goodness of a microscope because it shows objects *so wonderfully magnified*, when, in all probability, it will be found completely inert and ineffective when called upon to show them with a reasonable working power: thus converting what is really a proof of its worthlessness into an argument for its supposed superiority; for I myself shall ever maintain high magnifying power to be an evil, and that the less we can use of it the better, for it is sure to diminish the quantity of the object within the angle of visible field; to attenuate and dilute light and distinctness; to prevent us from having more than a point of an object truly in focus at once; and, finally, to render our instrument tremulous and totally unmanageable, to say nothing of the injury done to the observers' eyes by the strain they must suffer from difficult vision.

In order to ascertain the equality of power, there is no method more sure or effectual than to employ the dynameter

* I call a compound microscope effective when it shows everything which can be seen with single-lenses power for power, without the coloured fringes and fog of the latter.

in the way I have discovered, and described in my paper on the Amician microscope. Another means may be also recommended, which is to measure with the same instrument the size of the image of some distant body, such as a window, &c., formed in the visual pencils, taking care that the instruments are at the same distance from the object when the experiment is made. As to ascertaining power by any means which takes into consideration any standard of sight, it will be always subject to disputes, because every man has a standard of his own; and therefore, on this principle, it may be asserted that the magnifying power of a microscope is in fact different to every different observer. I myself generally examine a minute microscopic object at about four and a-half inches distance from my eye; another cannot see it nearer than eight or ten inches: so it may be said that the microscope magnifies as much again to him as it does to me. On this account it would be much more convenient for observers to mention the focus of the single lenses they use instead of their powers; and, if they use a compound instrument, *to state the focus of a single lens to which its power is equal*, according to the method first employed by myself, and which has been already sufficiently described. Not only, then, are the powers to be equalized, but, in compound instruments, the length of the body and the focus of the object-glass or metal must be also the same; for the penetrating power *cæteris paribus* perpetually increases along with the depth of the objective part. This is an eternal argument for object-glasses and metals of short foci, used with shallow eye-pieces. I shall thus explain my meaning:—suppose I try three instruments against each other, viz. a common compound microscope, having an object-glass of $\frac{1}{10}$th of an inch focus and $\frac{1}{20}$th of an inch aperture, and such a body and eye-piece attached to it as shall raise its power to 240 (or that of a lens of $\frac{1}{30}$th of an inch focus). I have also an achromatic instrument, with an object-glass of 0·4 focus and 0·2 aperture raised to the same power of amplification by an adequate eye-piece or an increased length of tube; together with an Amician reflector, having an objective metal of 0·9 focus and 0·45 aperture also raised to the same power: let us suppose, for ar-

gument sake, (it must be recollected I do not assert the fact,) that the three instruments showed some difficult proof-object equally to the satisfaction of the same observer; is this to show that the three instruments are equally good? Most assuredly not; quite the reverse. The experiment would demonstrate that the principle of the instrument which had the objective part of the least natural penetrating power, was decidedly superior in doing as much with 0·9 focus as the achromatic with 0·4, and the common compound with 0·1. In order to give to each its true position with regard to the others, let the objective part in each be rendered of the same focal distance, of the same angle of aperture; and the relative capabilities of the three instruments must then necessarily be developed. As Amician metals and achromatic object-glasses cannot well be made of 0·1 focus, let the common object-glass be elongated to 0·9, and the achromatic also; then compare them together, and a glance will suffice to show them in their true colours.

In fact, nearly all the test objects are just visible with a single lens of $\frac{1}{10}$th of an inch focus and the requisite aperture. It is, therefore, not very meritorious in the common compound to do as much as its object-glass will *by itself*, considering that the power gained by attaching the body to it will triple what it was in its natural, unassisted state. The excellency of the other instruments consists in their being able, when only equal in power to the said single 0·1 of an inch, to equal and indeed surpass it in point of performance. As to what a common compound will do with the same power obtained from an object-glass of 0·9 focus, and 0·45 of aperture, I think I need not mention, for its image will be a complete mess of aberration of both kinds.

The common instruments act much better, in proportion, with very deep object-glasses, having large angles of aperture, than with shallow ones in the same predicament; because the diameters of the former will necessarily be exceedingly small, and therefore admit so small a quantity of light, that however violent their aberration may be, but little becomes sensible, for as colour is nothing but decomposed light, where there is but little to be decomposed,

little can be seen: in the same manner, the fog resulting from spherical aberration consists of false and wandering rays not brought to a focus; but where there is little light to be scattered, little fog can be generated*.

It, however, must never be forgotten that it is no merit in a compound microscope to show objects which are to be seen much better with its object-glass alone, than with the help of the body attached to it. It is no wonder that the vilest machines ever turned out of the glazier's hands should show test-objects when their object-glasses have $\frac{1}{10}$, $\frac{1}{15}$, $\frac{1}{20}$, or $\frac{1}{30}$ of an inch focus with large apertures, because such lenses can by their own unassisted natural power show everything which can be seen by microscopes of any kind whatsoever.

Now Mr. Tulley's curves are published, doubtless, every optician will have achromatic object-glasses for microscopes as plentiful as blackberries, and at very reasonable rates too; a brace of convexes of crown glass or plate will have a concave of flint inserted between them; the composition will be called achromatic, and it will be very hard if the uncorrected aberration cannot be conquered by a reduction of the aperture, since that of a common lens may. A stop of very small aperture will be placed within the tube to effect this valuable purpose. Thus, this noble invention will get a bad name, and such achromatic object-glasses will be damned by all men of discernment as no better, or perhaps worse, than common lenses, just as Mr. Tulley's first attempts were by Mr. Troughton; but if my readers will do me the honour to attend to this disquisition on test objects, it will be in their power to examine the pretensions of all microscopes whatsoever, with infallible accuracy, and the unblushing effrontery

* The aberration of all small lenses must, of course, be very small, when considered as a *measured quantity*, compared with that of larger ones; but as it will always bear the *same ratio to their focus with any determinate angle of aperture*, I do not think their superior distinctness to be owing to this circumstance, but purely to the rarity or thinness of their light, or the few rays which have to co-operate in the production of their images; in confirmation of which position, I may observe, that it is notorious to all persons conversant with optical instruments, that the chromatic and spherical aberration of both telescopes and microscopes becomes much more sensible when objects are viewed by them in a strong and intense illumination, than when seen in a dull and faint one.

of the *shoptician* will in vain attempt to palm worthless trash on the ignorance of the public. I here, therefore, enter into the description of the figures in the plate :—i., ii., iii., and iv., have been already sufficiently described*; I shall therefore merely remark that *fig.* 1 is a true drawing of the object it is meant to represent, having been originally executed by Mr. Lister with a camera lucida eye-piece ; therefore, my readers will conclude that if they do not find the feather of a supposed Meneläus' butterfly, procured near the origin of the wing, to resemble it pretty closely, they have not obtained the same object, more especially if the apertures required to develop its peculiar structure do not correspond with my statement, for this will be the best security for its identity.

I am indebted to Mr. Carpenter's profound erudition in that branch of natural history which belongs to the microscope, for the ascertainment of the veritable butterfly to which this feather belongs: my readers may rest assured that it is the Meneläus. Mr. C. has a very fine one in his valuable cabinet.

Fig. 5.

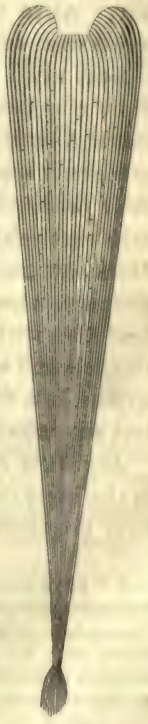


Fig. 5, is a drawing of another feather; it was taken by Mr. Tulley from some fragments of wings of foreign insects. Mr. Carpenter has discovered a specimen very similar to it, found on the upper surface of the under wing of the male papilio brassicæ. It does not show the curved lines at the double head, and is rather a more difficult object than the sample drawn. It is about $\frac{1}{1000}$ of an inch long by $\frac{3}{1000}$ or $\frac{4}{1000}$ broad, of a light brownish, yellow tint, and extremely transparent. In the plate the lines have too much strength and continuity, and are not sufficiently close; their traces are faint, uneven, dotted, and interrupted here and there. They are not resolvable without

* Vide the paper and copperplate in the last number of the same old series.

an enormous angle of aperture—none of the achromatics singly exhibit them satisfactorily. Mr. Lister's 0·3 does more with them than any other; but it may be considered lame and impotent, and rather giving indications of the lines than showing them outright*. When we reflect that they are not to be made out by single lenses, unless their aperture is somewhere about equal to their focus, it is surprising that the achromatics should show so much of them as they do, with an aperture not more than equal to half their focus. An Amician reflector, having an object metal of 0·3 focus with 0·2 of aperture, shows them completely well, with the cross fibres much more numerous than in the plate; and one of 0·6 focus with 0·3 of aperture, with a very oblique illumination, gives very strong indications of them, if it does not actually demonstrate them fairly drawn out. As to the reason why this class of objects should require such enormous angles of aperture to resolve their lines, I must say I am completely mystified. It is easy enough to show why nebulæ and clusters of stars should require an immense aperture to render them visible; but why mere lines extremely similar to those on micrometers (if we may judge by their prismatic effect and general appearance) should require such vast calibres, when the artificial ones are in no such predicament, must be admitted to be very perplexing:—the only property they seem to possess in common, is that of being best seen with an oblique light: the lines on glass micrometers, viewed as transparent objects, do not indeed totally disappear in a direct light through the axis of the microscope, but are infinitely better seen when it comes askance.

Fig. 6, *a* and *b*, are two more feathers which seem to be in the same condition with the last, as far as regards the aperture required to show them: they were procured from the

* I have seen them indifferently well with a 0·3, which was in a very imperfect state, and scarcely fit to act upon opaque objects at all. By combining two aplanatics together, a sufficient aperture is obtained to bring them out; but it is a way of effecting the purpose which ought to be avoided if possible. Mons. Chevalier combines two of his cemented double object-glasses together, which in my opinion form the best possible combination of this sort, giving a much more brilliant image than triple ones treated in the same way, which, from the number of their surfaces invariably produce great dulness in the image.

under side of the wings of those minute-brown moths which are found among clothes: Mr. Tulley first discovered them. They are generally mixed with others exactly resembling them in general appearance, but distinguishable from them by their lines being visible with a much smaller angle of aperture, which is always to be regarded as the discriminating test of identity with this class of objects. This feather (*a*) is about $\frac{3}{1000}$ of an inch long by $\frac{2}{1000}$ broad. In viewing these objects with single lenses, it must not be forgotten that their aperture is regulated by the dimensions of the hole in the anterior part of their setting, or that which comes next the eye, which must be somewhere about equal to their focus to enable them to demonstrate these lines; that is to say, the cylindrule of light which proceeds from them, measured by the dynameter, must be $\frac{1}{20}$ of an inch in diameter, if such be the focus of the lens employed. The size of the hole in the exterior part or cell may be much smaller than that of the internal without affecting the dimensions of the said cylinder from the diverging nature of the rays. It merely, indeed, determines the size of the field of view.

Fig. 6.

Fig. 7. The public has been long acquainted with the scales of the diamond beetle as a beautiful opaque object, without being aware of the valuable occult properties they possess as test objects. The mere true definition of the outline of one of these brilliant bodies on its dark ground, as an opaque object, by a microscope of *large aperture*, is a pretty good security for its quality. The lines, however, when seen upon them, as represented in the plate, are infallible evidence of a very considerable and effective aperture, combined with the requisite distinctness. They require precisely the same management of the light to be seen, both as opaque and transparent objects. When intended to be viewed by intercepted light, they must of course be scraped off and placed between talcs. They are not, when treated in this way, so severe a test as when seen by radiated light, but still excel-

Fig. 7.

lent proof objects. It is requisite for me to observe, that some of them do not seem to be striated or furrowed with lines at all, and there seems to be a good deal of difference in the difficulty with which different specimens are resolved into lines. The scales of many other brilliant foreign curculios also form excellent test objects, but not in general so difficult as these. There seems to be nearly as much difference among them as to the facility of revolution as among clusters of stars. I have seen some so easily developed, and requiring so small an angle of aperture, as to be distinctly visible with the most ordinary compound microscope. I shall take leave of this class of objects by observing generally, that no accumulation of condensed artificial light on them seems to have any operation in supplying or compensating for want of aperture. When we have an instrument possessing this one thing needful in the requisite state of perfection for the purposes of vision, it is surprising in what a faint light all the family of lines may be seen, both as opaque and transparent bodies: on the contrary, without this essential virtue in the instrument used, we in vain look for lines even when we have rendered our field of view in a complete blaze with condensed rays. It is not mere light then that shows them, but the due breadth of the visual pencil, with respect to the power employed. The Amician reflector (beyond comparison the dullest and darkest under any given circumstances of the whole family of microscopes) shows these said lines infinitely better than any other instrument. The achromatics show them very well also, especially the thin 0.3. Single magnifiers are very inert upon them *as opaque bodies*, because the silver cups in which we are compelled to insert them to gain the power of seeing opaque bodies do not easily admit of that obliquity of illumination which seems one postulatum for their exhibition.

Equally unavailing to the demonstration of these objects is mere magnifying power, however great even when obtained from the deepest object-glasses, (though I have already had occasion to remark that it is much more valuable, effective, and penetrating when procured in this way than from the operation of deep eye-glasses on shallow objectives,) unless it

is accompanied by the proper calibre. I am not aware that a magnifier or object-glass of $\frac{1}{60}$ of an inch focus, and $\frac{1}{300}$ of an inch of aperture, will perform any better than one of $\frac{8}{10}$ focus and $\frac{1}{25}$ of an inch of opening, which would possess the same ratio or angle of aperture. As there are always various degrees of vision in difficult objects of all sorts in optical instruments, the said lines may be seen with more or less facility with different object-glasses and instruments, with more or less coaxing and management of the light; with more or less stability in the stand; with more or less accurate adjustment of the focus, &c. &c.

There is, however, a way of determining which is the best instrument, when two will show an object, but when there is some doubt as to which shows it the best; it is this: convert the instrument into a magnifier for a solar microscope, and see which will imprint the best image on a sheet of paper (for such as the microscope is, such will be the image formed by it). That which gives the strongest and most vivid picture will be sure to perform best when treated in this way; for it requires no little efficacy in a microscope to show a difficult and delicate object fairly depicted on paper, all faint and diluted rays being in this way utterly lost and extinguished. With the solar microscope I have alluded to in the former paper, and the 0.2 object-glass attached to a compound body giving it power equal to that of a single lens of $\frac{1}{25}$ of an inch, and the amplification of the image in the camera being confined to 200, I have actually procured an image of the lines in the diamond beetle scales as *transparent objects*, and of course of every other variety not more difficult than these; but with the 0.933 object-glass, *cæteris paribus*, I never could procure an image of any but *fig.* 1, or of other lines of the same class and resolvable with the same ease; though the said 0.933 will show *faintly* (when treated with an eye-glass only as a compound microscope) almost every object which can be seen by the 0.2.

No difficult opaque object whatever can be exhibited by the solar apparatus, such as any of the family of lines, as opaque objects, the minutiæ of a fly's foot, or of a mouse hair, &c., though a most beautiful and pleasing picture of

ordinary objects is given. I need scarce say that *figs.* 5 and 6, and the whole of that stubborn and indomitable race to which they belong, utterly resist the penetration of the single solar achromatics. As to the amplifying power necessary to demonstrate the lines on the feathers, I may state that *figs.* 1 and 7 begin to be barely visible in a very perfect instrument and with very close attention, with a degree equal only to that of a lens of $\frac{1}{6}$ of an inch focus; but *figs.* 5 and 6 require one equal to $\frac{1}{12}$ of an inch at least.

Fig. 8.

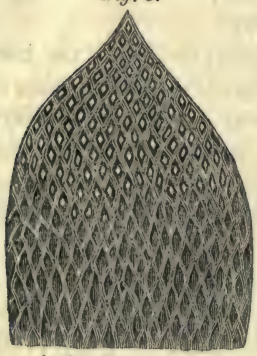


Fig. 8 is a drawing of the point of a leaf of hedge moss (I am sorry I am at a loss how to designate it scientifically): it is a very good test object, requiring a considerable angle of aperture combined with great distinctness fully to develope the lozenges which constitute its fabric, making out a luminous nucleus to each, which should be sharply defined and of the same shape with the outer lozenge, and not melting into it by insensible gradation as in the copperplate engraving. The Amician microscope is sure to boggle at this subject, if the figures of the metals are not good. All mosses exhibit more or less of this structure; but the specimen in question is remarkable for the prominence of its features. The engraving being very exact, except in the trifling circumstance I have mentioned, any specimen tried and not found to resemble it must be set down as another sort: the wood-cut in the margin is correct.

Fig. 9.

Fig. 9. Bat's hair.—The hair of the bat near its insertion in the skin often shows an outline similar to that in the plate, being apparently the result of a certain spiral coil wound about it: frequently, however, it shows an appearance similar to that produced by the insertion of a number of funnels into each other. When well defined as a transparent object by a microscope of large aperture, it is a sure test of its distinctness. I have given it as an opaque object, in which way it is far more difficult of vision than as a diaphonous one. The

power required to show it well must be equal to $\frac{1}{20}$ of an inch; a very strong artificial illumination is also requisite: it does not show well upon a dark ground, but on one somewhat transparent, for which reason a little intercepted light behind it is useful. When treated in this way, the coil may be seen verified, and as I have drawn it, as a transparent object, day-light shows it better than lamp or candle light.

Fig. 10. Mouse's hair;—another object of the same class: *a*, opaque, *b*, transparent. Both their varieties occur in the same animal, as represented. The large hairs are punctuated with circular indentations passing spirally round them: they may be distinctly demonstrated as opaque objects. The difficult point is to see them so verified by oblique illumination that they appear like little cups shaded as they are when exposed to the same light: a black ground is absolutely necessary to bring out their structure satisfactorily. They may also be seen very well, as transparent objects, by ordinary compound microscopes with the usual small angles of aperture; but when seen distinctly with a *large aperture* they are a good test: achromatics will sometimes show the whole tribe of lines without being able to make them out. *b* is the small mouse hair, seen as a transparent object; it is chequered by circular annuli, between which are to be seen longitudinal fibres, which last are the test, not being visible unless by a great deal of good distinct light. Mr. Cuthbert first discovered them with an Amician reflector, which, if really good, shows them better than any other microscope will.

Fig. 10.

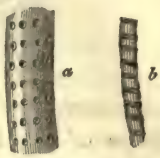


Fig. 11 is merely a magnified piece of watch plate, with letters enamelled white on a black ground. It is Mr. Tulley's favourite object for working all his object-glasses; by this he judges of the state of their achromatic and spherical aberration, adjustments, &c. Considered as a mere microscopic object, it is an exceedingly easy one, being visible to the eye; any common compound microscope will of course show it fast enough, if its object-glass is cut off and reduced in aperture till its aberration becomes insensible. The diffi-

culty, however, consists in showing it with a *large aperture in the same way as drawn at* a, *with the black and white distinctly separated from each other.* If there is any spheri-

Fig. 11.

cal aberration about the object-glass, it shows itself in the shape of a nebulosity about the confines of the black and white, as at *b*; or if worse still, in a diffused nebulosity or fog over the whole of the plate, as at *c*. The achromatic aberration also shows itself in a fringe of colour mixed up with the fog or nebulous border. There is frequently another appearance which I have not represented; it consists in a sort of penumbra or double image which cannot be made to lap over the other by any adjustment of the focus, but still shows itself in a sharp but faint margin to the true outline of the figures: reflecting microscopes, if their metals are not truly figured, are very apt to show this flaw in performance.

We may rest assured that if any object-glass or metal defines this object completely well, *with such an aperture as will also show the lines on the feathers*, it must be good. An objective-glass of 0.933 focus ought to be able to do this with an aperture of 0.38, or even 0.4, if in good adjustment; others of shorter foci with a proportional angle at least. Mr. Cuthbert's metals of 0.3 focus will define it with an aperture of 0.2, which no single aplanatic will ever be made to do. A very excellent proof object for achromaticity may be made of a piece of fine wire-sieve (if dirty so much the better), treated with intercepted light thrown obliquely through its interstices. This is sure to surround the wires with coloured fringes; and by putting them within and without the focus, the various rays will show themselves in the order of their refrangibility: however contracted the aperture of a common object-glass

may be, an experienced eye will always be able to see the uncorrected achromatic aberration by this test. Small holes drilled in a thin metallic plate and blacked, form a similar object, and generally all dark bodies seen against intercepted light show the same phenomena.

A very valuable object to the working optician is an artificial star; this may be made by a very minute glass globule stuck on a black ground, or by some grains of platina fused by electricity. Mr. Lister first employed globules of quicksilver for the same purpose.

His way of procuring them is, I believe, as follows:—

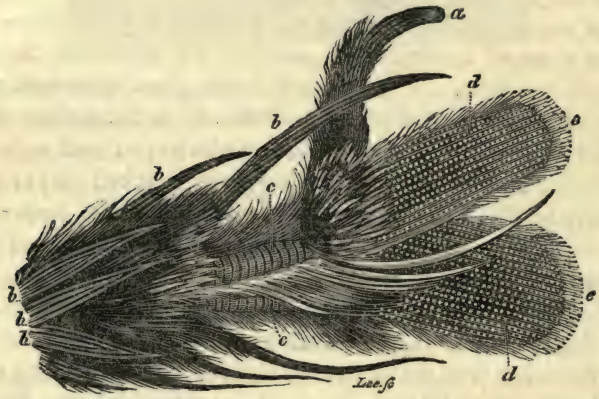
Let a small quantity of mercury be squeezed through leather, to clean it; then put it into a glass tube with some water having a very little gum dissolved in it; agitate it till it resolves itself into very minute globules; extract a few of them, and let them be secured on some blacked paper with the help of a little thin gum-water; they may then be crushed with the finger so as to be totally invisible to the naked eye: the smaller they are the better.

The light of the sun falling upon them produces a beautiful disk, but common day or candle light does pretty well also. The *initiated* well know how to appreciate the use and value of this object, which is, perhaps, the best which can be employed to verify the state of the figure, achromatism, centreing, grinding, and adjustment, &c. of the small aplanatics. All their defects may be clearly looked into and probed by the means of this single object, and the proper remedies applied to them, which, of course, cannot be done unless the nature of the disease is duly ascertained.

Fig. 12 is the foot of a blue-bottle fly, an interesting opaque object, and an admirable test, its minutiæ requiring a great deal of good distinct light, combined with considerable power to become duly manifest. The specimen I have drawn is, I believe, one of the hinder feet, for the three pair all differ somewhat from each other in the proportion of their component parts, though alike in their general structure: (as one of the claws was buried in the gum which was used to stick the foot on the black surface to which it was applied, I have not represented it.) I lament moreover that my draw-

ing must be considered as having been taken from a dried specimen instead of a recent one, (as it should have been,) for it is an object which is sure to be much changed, warped, and shrivelled up, if detached from a newly-killed fly, even for one hour only. However, as the prominent features which constitute the peculiar subjects of examination as tests were well preserved, the drawing will do well enough to point out what I wish to submit to my readers' consideration.

Fig. 12.



At *a* is to be seen a fluting in the claw; *b*, *b*, *b*, *b*, *b*, are lines, furrows, or flutings in the hairs of a whitish tint; *c* is a species of scale-work on the shank somewhat similar to what may be observed on the feet and legs of birds; *d d* are minute white dots, probably the orifices of glands, dispersed over the whole of the inferior surface of the suckers as represented in the plate; *e* is a fringe of hairs round the margin of the suckers, with minute globules at their terminations.

Now all these minutiæ can be seen, in more or less perfection, in every good specimen. I must observe, however, of the dots or granulations on the soles of the feet, that they are sometimes not visible, owing to the whole surface being occasionally furred over with dust, dirt, and other extraneous matter, which is apparently licked up by the glutinous fluid which exudes from the orifices of the glands. I think that kind of light which is afforded by a silver cup does best upon

the whole for this object, and is, indeed, absolutely necessary to give the Amician reflector the power of showing this object advantageously, though I have managed to see every thing there is to be seen about a foot, without its assistance to the effect of the said instrument. I may remark, that, being a very sombre object, it is peculiarly calculated to be seen unfavourably in the dark image of the reflector; nevertheless, every point can be shown by the Amician metals of 0·6 focus and 0·3 aperture completely well, and with as much ease too as by any other instruments greatly superior to them in mere light or brilliancy of effect. A dark ground of some kind or other is absolutely necessary to exhibit this object in its proper relief; a white one confuses it altogether; the power may vary from $\frac{1}{10}$ to $\frac{1}{30}$ inch: candle or lamp light, I think, does best for it, and the darker the apartment is made in which we observe it the better, for darkness causes the iris of the eye to expand, and receive a stronger impression from the faint rays issuing from such dull objects.

A human hair is a beautiful proof object; I repent that I have not given a figure of it.

Mr. W. Tulley has discovered a singular property in almost all hairs, which I do not think has ever been remarked or described. If a hair is drawn between the finger and thumb from the end to the root, it will be distinctly felt to give a greater resistance and a different sensation to what is experienced when drawn the opposite way; in consequence, if the hair is rubbed between the fingers it will only move one way, (travelling in the direction of a line drawn from its termination to its origin from the head or body,) so that each extremity may thus be easily distinguished even in the dark by the touch alone.

The mystery is resolved by the microscope. A hair viewed on a dark ground as an *opaque* object, with a high power not less than that of a lens of $\frac{1}{30}$ of an inch focus, and dully illuminated by a *cup*, which seems to answer best, is seen to be indented with teeth somewhat resembling those of a coarse round rasp, but extremely irregular and rugged; as these incline all in one direction, like those of a common file, viz. from the origin of the hair towards its extremity, it suffi-

ciently explains the reason of that singular property I have described.

This is a singular proof of the acuteness of the sense of feeling, for the said teeth may be felt much more easily than they can be seen. We may thus understand why a razor will cut a hair in two much more easily when drawn against its teeth, than in the opposite direction.

P. S. The instrument which I have in this paper termed the *Amician Microscope* is not the legitimate offspring of Amici, (from whom its *principle only* has been borrowed); but an improvement of mine, executed by T. Cuthbert. The profound critical acumen and superior taste of the professor (as I am given to understand) wholly rejects and disowns the particular modification in question, as a spurious and bastard variety, which must be affiliated upon myself, being wholly different from his in every thing but the optical principle. With this decision I am perfectly content.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

July, 1827.

i. *A Letter from* Mr. Henderson, *containing a corrected Method of computing an observed* Occultation.

Dear Sir, *Edinburgh, 3, Leopold Place, May*, 1827.

I think that my Rules for computing an observed Occultation may be improved; and, in order to enable you to judge of this matter, I beg leave to inclose a copy of the corrected rules with an example. I give the preference to the corrected rules for the following reasons:—

1. When the star is near the zenith, a considerable degree of uncertainty attends the altitude computed by the method in the Requisite Tables, on account of the small variations in the sines of arcs approaching to 90°: and this uncertainty exercises a considerable influence upon the parallactic angle

and parallaxes, as I have several times remarked in cases which have actually occurred.

2. The affection of the parallactic angle is sometimes ambiguous, as noticed in Remark 1.

But, in the corrected rules, these imperfections are avoided by computing the parallactic angle by Napier's analogies.

3. By the rules in the N. A. the parallactic angle is directed to be reckoned sometimes from the North, and sometimes from the South Pole. But by the corrected rules, this angle is always reckoned from the North Pole, whereby the precepts are simplified.

Other changes have been made; and, if you think proper, the corrected rules may be published in such manner as you may deem suitable. And I am, with respect,

Dear Sir,

Your very obedient humble servant.

THOMAS HENDERSON.

Rules for computing an observed Occultation.

1. FIND half the sum and half the difference of the north polar distance of the zenith (corrected on account of the spheroidal figure of the earth), and the north polar distance of the star; and to the logarithmic cosecant of the half sum add the log. sine of the half difference, and the log. cotangent of half the star's horary angle; the sum is the log. tangent of an angle A always less than 90°: and to the log. secant of the half sum add the log. cosine of the half difference, and the log. cotangent of half the horary angle, the sum is the log. tangent of an angle B, which is greater or less than 90°, accordingly as the half sum is greater or less than 90°. If the north polar distance of the zenith be greater than the north polar distance of the star, the sum of A and B, otherwise their difference, is an angle C, to which prefix the sign + when the star is east of the meridian, and − when west. When the orbital angle has N. prefixed, its complement to 90° will have the sign −, and + when S. The sum of this com-

plement and of the angle C (observing the algebraical rule of the signs as in addition) is the complement of the parallactic orbital angle, or the complementary angle with its proper sign. When this angle exceeds 180°, its supplement to 360°, with the sign reversed, is to be taken.

Example.—At Port Bowen, on 3rd January, 1825, the immersion of η Geminorum was observed at $5^h\ 9^m\ 27^s$ apparent time.

	H.	M.	S.				
Apparent time	6	9	17	Latitude	73°	13′	40″ N.
Sun's A. R.	18	58	21	Reduction		−6	20
Sidereal time	1	7	38		73	7	20 N.
Star's A. R.	6	4	22	Zenith's N. P. D.	16	52	40
				Star's N. P. D.	67	27	4
Horary angle	4	56	44	Sum	84	19	44
	°	′	″	Difference	50	34	24
	= 74	11	0	Half sum	42	9	52
Half	= 37	5	30	Half difference	25	17	12

Log. cosec. half sum			0·17311	Log. secant			0·13005
—— sine half difference			9·63057	—— cosine			9·95626
—— cot. half hor. ang.			0·12144	—— cot.			0·12144
—— tang. A =	40	5	10	9·92512	Log. tang. B = 58°	12′	30″ 0·20775
B =	58	12	30				
C = +	18	7	20	(star east of meridian.)			

Comp^t. orbit. angle = + 5 35 0 (orbital angle S.)
Complement^y angle = + 23 42 20

2. Add together the log. cosecant of the horary angle, the log. cosecant of the north polar distance of the zenith, the log. sine of the angle C, the log. secant of the complementary angle, and the proportional logarithm of the moon's horizontal parallax, corrected for the spheroidal figure of the earth; the sum is the proportional log. of the perpendicular parallax, which has the sign + or −, accordingly as the complementary angle is greater or less than 90°. To the prop. log. of the perpendicular parallax add the log. cotangent of the complementary angle, the arithmetical complement of the prop. log. of the moon's horary motion, and the constant log. 0·4771, the sum is the prop. log. of a portion of time, which, added to or subtracted from the time of observation, accordingly as the parallactic angle (or its supplement, in the case mentioned above) has the sign + or −, gives the time corrected.

Example.

Log. cosec. hor. angle		0·0168
—— —— N. P. D. of Zenith		0·5371
—— sine C		9·4928
—— secant comp^{y} angle		0·0383
Prop. log. reduced hor. par.		0·4861
—— —— perpendicular parallax =	− 48′ 20″	0·5711
Log. cotang. comp^{y} ang.		0·3575
A. C. Prop. log. 35′ 20″ H. M.		9·2929
Constant log.		0·4771
	H. M. S.	
Prop. log. +	0 36 2	0·6986
Time of observation	6 9 17	
Time corrected	6 45 19	

3. The nearest approach will have the sign + or −, accordingly as the difference of declination at the conjunction is marked N. or S. The sum of the nearest approach and perpendicular parallax is the star's distance from the apparent orbit, which add to and subtract from the moon's horizontal semidiameter without augmentation. To half the prop. log. of the sum add half the prop. log. of the difference, the arithmetical complement of the prop. log. of the moon's horary motion, and the constant log. 0·4771, the sum is the prop. log. of another portion of time, which, added to or subtracted from the time corrected, accordingly as an immersion or emersion has been observed, gives the time of the nearest approach for the meridian of the place of observation; which being compared with the Greenwich time set down in the Elements, the difference of longitude is obtained.

Remark.—In some rare cases, when the star's distance from the apparent orbit is nearly equal to the semidiameter, the portion of time will have to be applied in a contrary manner to that here directed. This will happen when the immersion takes place after, or the emersion before, the apparent conjunction.

Example.

Nearest approach	+	50′ 42″		
Perpendicular par^{x}	−	48 20		
	+	2 22		
Semidiameter		16 4		
Sum		18 26	Half P. L.	0·4948
Difference		13 42	—— ——	0·5593
			A. C. Prop. log. H. M.	9·2929
			Constant log.	0·4771
	H. M. S.			
Prop. log. +	0 26 59			0·8241
Time corrected	6 45 19			
Time of nearest approach	7 12 18			
Ditto for Greenwich	13 8 3			
Longitude of Port Bowen	5 55 45	west of Greenwich.		

ii. *Remarks on the* SOLAR TABLES, by THOMAS HENDERSON, *Esq.*

The discordances, which Mr. South has observed, between the computed and the observed Right Ascensions of the Sun, may be chiefly accounted for by the following considerations:

De Lambre's Solar Tables, from which the computations of the Nautical Almanac are made, were published in 1806, but were constructed some years previously. He determined the Epochs and mean motion by a comparison of Bradley's Observations, about 1752, with Maskelyne's, about 1802, reduced by the Catalogues of Stars then most approved. Since the publication of these Tables, M. Burckhardt has compared the Epochs with the Greenwich Observations, and has reduced them by more correct catalogues, *viz.*, Bradley's Catalogue for 1756, computed by Maskelyne, and Maskelyne's latest Catalogue for 1805. In this manner he has found that the secular motion in De Lambre's Tables should be increased by 12″.2, and that the Epoch for 1801 should be increased by 0″.80. See his Memoir "Sur les Masses des Planètes," in the Connaissance des Tems for 1816, p. 341. Consequently the Epoch for 1822 is too little by $3''.3 = 0^s.22$ in time. But Mr. Pond's Catalogue, which Mr. South has used in his computations, exceeds the above Catalogues when reduced to 1822 by $0^s.31$; and therefore, from these two causes, the Sun's observed Right Ascensions ought to exceed those computed in the Nautical Almanac by $0^s.53$, which agrees very well with the quantity found by Mr. South.

Supposing the mean places and motions of the sun, moon, and planets relative to the stars to be well determined, if any change should afterwards be made upon the mean places of the stars, or rather of the equinoctial point, or upon the precession of the equinoxes, the same changes should be made upon the mean places and motions of the sun, moon, and planets, otherwise discordances must ensue. From what has been said it would appear, that the source of the discordances found by Mr. South lies here, and not in the imperfections of the Solar Tables. These discordances rather

seem to be a proof of the accuracy as well of the Tables as of the Observations.

[It may be added to these Remarks, that the accuracy of the comparison of the Solar Tables with the Greenwich Observations for 1820, as computed for the Board of Longitude, and published at the end of Mr. South's paper, is confirmed by the computations lately printed by the Astronomer Royal; the mean error of the tables being $-0^s.195$, according to the former comparison, and $-0^s.203$ according to the latter. Bouvard's corrections give a mean error of about $+0^s.05$. EDITOR.]

iii. *Answer to some Remarks of* Mr. IVORY. Astr. Nachr. No. 108. *Translated at the particular request of* Professor BESSEL.

I have been much surprised to find in the Philosophical Magazine for April and May, 1826, an Essay of Mr. Ivory, in which I am directly accused of having copied my Analysis of the mode of computing geodetical measurements from the Philosophical Magazine for July 1824.

Mr. Ivory ought to have considered that it is very unusual even to hint at so degrading an accusation, without having the power of showing its truth: and I hope to be excused for not undertaking to defend myself against an attack so wholly unsupported by evidence.

The dates of my three papers are December 1821 (A. N. 3), January 1822 (A. N. 6), and October 1825 (A. N. 86). I have never seen Mr. Ivory's paper of July 1824, and I know nothing of its contents: nor have I seen the 41st number of the Journal of the Royal Institution. With respect to my last paper, if it really contains what Mr. Ivory had before published, I readily give up my claim to priority; for I have always thought that the date of printing alone should determine the right to any scientific property.

But it is not so much the question to decide on this claim, as to repel an unjust accusation: I am therefore compelled to show the actual date of the researches in question. They were in fact laid before my pupils in a course of public lectures, delivered in the University of Königsberg, in the winter of 1821-2; and I have the pleasure of mentioning among my audience the names of *Argelander*, *Rosenberger*, *Scherk*, *Strehlke*, and *Klupsz*, all of which are already known in

the literature of astronomy. I imagine also, that every attentive reader will infer, from the form of my *second* essay, that I must have been already in possession of the materials of the *third* when I wrote it. So much for my personal explanation : but I have a few more remarks to make in answer to some further criticisms of Mr. Ivory.

The object of my third essay was to give a method, by which the Longitudes and Latitudes might be computed, not as far as the second or fourth powers only of the eccentricity, but as near as it was possible to compute them with any trigonometrical tables whatever : and this was to be done, not for short distances only, but generally to every possible extent of measurement : and I apprehend that I have effected this by a computation, which is not materially more laborious than if the figure of the earth were spherical. This computation requires the assistance of a table of four octavo pages, and which contains the coefficients, computed once for all, that it would without it have been necessary to determine in each particular case. How far Mr. Ivory is right in calling these four pages "*bulky tables*," and whether he is correct in saying that my expression for the portion of the geodetical line requires "*the calculation of many subsidiary arcs*;" I must beg every reader, who understands my Essay, to determine for himself. The truth is, that my tables require the computation of no subsidiary arc whatever, but merely the formation of two arguments, which are obtained by adding to the logarithm of cos m, which is always of necessity to be found in such computations, two other logarithms depending only on the eccentricity, and which are therefore always the same for the same spheroid. Whoever attempts to apply Mr. Ivory's formulas to a determination, equally accurate with that which is obtained from my tables, will very soon find on which side the advantage lies. Mr. Ivory appears therefore to have gone too far when he calls my method "complicated :" and I am the more disposed to think, since I have read Mr. Ivory's Essays, that it contains nothing whatever that is superfluous, upon the supposition that we wish to obtain perfect accuracy both for small and for great distances.

iv. *Elementary View of the* UNDULATORY *Theory of* LIGHT. *By* Mr. FRESNEL.

[Continued from the last Number.]

It is easy to convince ourselves here, as well as in Dr. YOUNG's experiments, that the fringes depend on the mutual actions of the rays which meet each other; for if we intercept, by means of a screen placed near to one of the mirrors, all the rays that it reflects to the place of observation, either before or after their reflection, the fringes wholly disappear, although the space continues to be enlightened by the other mirror, and we can only see some paler and unequal fringes which form a border to the shadow of the screen. If we cover with the screen only one half of the mirror, so as to annihilate the fringes for one half only of their entire length, we may the more easily compare the remaining part of the dark stripes with the neighbouring space which is enlightened by one of the mirrors only, and we may thus readily be assured that the neighbouring parts are much brighter than the middle of the dark stripes, on which, however, the rays from both the mirrors fall. These rays must therefore be neutralised in consequence of a certain action which they exert on each other.

This mutual influence of the rays of light, established by the experiments which have been enumerated, is still more fully confirmed by a great number of optical phenomena; so that it may now be considered as one of the best demonstrated of physical truths. After having thus satisfied ourselves of its direct foundation, we may proceed to its confirmation by the most important collateral observations. But we must first examine the circumstances under which the law takes effect.

If we calculate the differences of the routes described by the rays which concur in the production of each of the dark and bright stripes, we first find that the bright stripe in the centre corresponds to an equality in the length of the routes: and that if we express by d the differences of the routes described by the rays of the pencils which unite in the middle

of the next bright stripe on either side, the middle points of the other stripes will correspond to the differences $2d$, $3d$, $4d$, $5d$, $6d$, and so forth; while the middle points of the dark stripes, from the first to the most remote, correspond successively to differences in the routes, expressed by $\frac{1}{2}d$, $\frac{3}{2}d$, $\frac{5}{2}d$, $\frac{7}{2}d$, and so forth.

Hence it follows, that the union of the rays produces the maximum of light, when the difference of the paths, which they have described, amounts to 0, d, $2d$, $3d$, $4d$, $5d$, and so forth; and that, on the contrary, they neutralise each other, and produce darkness only, when this difference is equal to $\frac{1}{2}d$, $\frac{3}{2}d$, $\frac{5}{2}d$, $\frac{7}{2}d$, $\frac{9}{2}d$, $\frac{11}{2}d$, and so forth. Such is the general law of the periodical influences which the rays of light exert on each other.

When the two luminous pencils are of equal intensity, as in the experiment just described, the middle of the dark stripes exhibits a total absence of light, at least in the fringes of the first and second, and even of the third order, if the light employed is sufficiently homogeneous; but, as it is never completely so, it happens that the inequality of brightness of the dark and light stripes, which is so prominent in the first fringes, diminishes gradually as we go further from the middle, and at a certain distance becomes insensible. The reason is easily understood; the light employed, however simple it may have been rendered, either by decomposition in a prism, or by its passage through a coloured glass, is always composed of heterogeneous rays, of which the colour and the physical properties differ but very little, although the period of interference has not precisely the same length: hence it arises, that the dark and bright stripes, of which the situation depends on the period, are not separated by precisely the same intervals. In truth, the breadths of the fringes, produced by the heterogeneous rays, differ so much the less, as the light employed approaches the more to perfect homogeneity; but however small this difference may be, it may be imagined, that, when repeated a great number of times, it will at last produce such a difference in the situation of the fringes, that the dark stripes of some of them will coincide with the bright stripes of others; so that at a

sufficient distance from the middle, where the two routes are equal, the dark and light stripes of the different species of rays of light employed will efface each other mutually by their mixture, and will exhibit a uniform tint.

The more homogeneous the light has been rendered, the more distant from the middle will this point of perfect compensation be, and consequently the greater number of fringes will be perceptible. When we employ white light, which is the most compound, the number of fringes visible is also the least possible, and we scarcely discover more than seven on each side of the centre. They exhibit the tints of the coloured rings, and the reason of their appearing coloured is exactly the same. If the length d were equal for the rays of different colours, the breadth of their fringes, that is to say, the interval between the middle of two consecutive bright or dark stripes, being also the same, there would be a perfect coincidence in their darkest as well as their brightest points; and the different rays which compose white light being found every where in similar proportions, would produce a series of black and white stripes without any distinction of colour. But this is by no means the case: as the length d varies very considerably for rays of different colours, that is, nearly in the proportion of two to one, from one extremity of the solar spectrum to the other, the breadth of the fringes varies in the same proportion, so that their dark and bright stripes cannot fall on each other, but differ so much the more in their situations as they are further removed from the middle line. It must therefore happen, that the bright stripe of the rays of a certain colour corresponds with the dark stripe of the rays of another kind, whence arises the predominance of the former, and the exclusion of the latter. Thus the fringes will exhibit a succession of tints, varying according to the unequal proportions, in which the different rays contained in white light will be mixed.

The middle line of the whole system is always white, because, since the difference of the routes there vanishes, it affords the maximum of brightness for all kinds of rays, whatever the length of d may be. On each side of this

white stripe, the light becomes gradually coloured; the colours of the second fringe are very distinct, as well as those of the third and fourth; but they afterwards become fainter; and they totally disappear after the eighth fringe, by the more complete mixture of the dark and bright stripes of all the colours, which produces a uniform white light.

If we make the experiment, which has been described, with the rays of the seven principal colours which Newton distinguishes in the solar spectrum, and measure the breadth of the stripes with the assistance of the micrometer already mentioned, it will of course be possible to deduce from it, by calculation, the corresponding values of d; but this observation has only been made with care on the red light, tolerably homogeneous, that is transmitted by certain coloured glasses used for the windows of churches. For the rays which preponderate in this light, and which belong to a part very near the end of the spectrum, the length of d is .00002512 inch, English. We may derive the value of d, for the seven principal species of rays, from the observations of Newton on coloured rings; it is sufficient for this purpose, as will hereafter appear, to multiply by 4 the lengths of what he calls fits of easy reflection, or of easy transmission of the luminous molecules. In this manner the following table has been calculated:—

Limits of the principal Colours.	Extreme values of d.	Principal Colours.	Intermediate values of d.
Extreme violet	.0000160 i [.0000167 Y]		
		Violet	.0000167 i [.0000174 Y]
Violet indigo	.0000173 [.0000181]		
		Indigo	.0000177 [.0000185]
Indigo blue	.0000181 [.0000189]		
		Blue	.0000187 [.0000196]
Blue green	0000194 [.0000203]		
		Green	.0000202 [.0000211]
Green yellow	.0000210 [.0000219]		
		Yellow	.0000217 [.0000227]
Yellow orange	.0000218 [.0000235]		
		Orange	.0000230 [.0000240]
Orange red	.0000235 [.0000246]		
		Red	.0000244 [.0000256]
Extreme red	.0000254 [.0000266]		

[The numbers within brackets are those which Dr. Young inserts in his table as derived from Newton's experiments.]

The explanation, which has been given, of the small number of fringes produced by white light, and of the limited number which are perceptible in the most homogeneous light that can be procured, is sufficient to inform us, why, in many cases of the crossing of the rays derived from common sources in directions nearly parallel, there is no production of fringes whatever: since the difference of the paths described is too considerable, and is too great a multiple of d, at all the points of the space enlightened by the two pencils united; so that the middle of the fringes, and the stripes near enough to it to be perceptible, correspond to points without the common field of the double illumination.

This is the reason why it is so important, in the experiment with the mirrors, that they should not project one beyond the other; for, on account of the extreme minuteness of the quantity d, which is only about one fifty thousandth of an inch for the yellow rays, the slightest inequality of projection, producing a difference in the length of the path equal to twice its own magnitude, may throw the group of visible fringes beyond the common field of the two mirrors. It may also be observed, in such experiments, that besides the rays regularly reflected by the mirror, there are always some which are inflected in the neighbourhood of their terminations, and which enlarge in this manner the space of the double illumination. The rays regularly reflected by one of the mirrors, interfering with the rays inflected near the circumference of the other, may also produce fringes, when the difference of the paths described is very small; but these fringes are distinguished in general from those which depend on the interference of the rays regularly reflected, by their curvilinear form, and by their direction, which is not perpendicular to that of the line joining the two images of the luminous point.

The reasoning, which has been employed for explaining the colours of the fringes produced by the mutual influence of two white pencils, may be applied to all the phenomena of diffraction in white light. These effects depend always on the difference of the breadths of the dark and bright

stripes belonging to the different colours, which prevents their existing at any one point, in the proportions required for constituting white light. The situation of these stripes, being known for each species of rays, as well as the laws, according to which their intensities vary in their different parts, it will be possible to calculate the proportions of their mixtures, and then to determine the tints that they compose, by means of the empirical formula of Newton, which enables us to find the tint corresponding to any given mixture of coloured rays. Thus it is sufficient to study the optical phenomena in any homogeneous light, which presents them in their simplest form, and it will always be easy to infer from them the appearances which must be produced, under similar circumstances, in white light. Henceforwards, therefore, it will always be supposed that we employ a homogeneous light, except when the results of experiments made with white light are expressly mentioned.

It may easily be inferred, from the very simple law of the influence of the luminous rays, which has been explained, that the breadth of the fringes [in order to preserve the same value of] d, must be in the inverse ratio of the interval which separates the two images of the luminous point, and in the direct ratio of their distance from the micrometer, or, in other words, must be in the inverse proportion of the angle under which the observer would see this interval, if he placed his eye at the point where the fringes are reversed.

The same geometrical law is applied to the fringes produced by two very narrow slits cut in a screen. The breadth of the fringes, which they exhibit, is in the inverse proportion of the interval comprehended between the middle points of the two slits.

This law is also true, in an approximate degree, of the fringes which are observed in the shadow of a narrow substance, at least until they approach near to the limits of the shadow; for when this is the case, they are governed by a more complicated law, which nevertheless depends on very simple principles, although it can only be represented by a transcendental function, containing, besides the breadth of

the body and its distance from the micrometer, its distance from the luminous point.

With respect to the exterior fringes which surround the shadows, their breadth depends always on both these distances: the former remaining constant, they are so much the broader as the second is smaller.

When, on the contrary, the respective positions of the luminous point and the screen are invariable, and the distance of the micrometer only from the screen is changed, we observe that the breadth of the external fringes is not proportional to it, as that of the internal fringes was. This fact may be expressed in a more geometrical manner by conceiving a right line drawn through the luminous point, in the direction of a tangent of the opaque body, so as to constitute the limit of the geometrical shadow, and saying that if we follow in space the middle of the same dark or bright stripes, and let fall from this point, at each distance, a perpendicular on the tangent, we find indeed that this little perpendicular increases as we go further from the opaque body, but in a less proportion than that of the distance from that body. Hence it follows that the same point of a dark or light stripe, belonging to the external fringes, does not describe a right line, but a curve, of which the convexity is turned outwards. This may be demonstrated by a precise measurement, made by means of the micrometer that has been described: and the result being a very remarkable one, it will be worth while to describe one of the experiments by which it has been exhibited: an experiment which was made with light apparently homogeneous, that was transmitted through the red glass already mentioned.

The opaque body being at the distance of 118.53 E. I. from the luminous point, I measured the successive intervals between the edge of the geometrical shadow, and the darkest point of the dark band of the third order: first at the distance of .067 i. from the opaque body; secondly at 39.49 i.; and thirdly at 157.28 i.; and I found for the first interval .00315 i., for the second .0866; and for the third .2296. Now if we join the two extreme points by a

right line, we shall find .0599 for the ordinate of that line corresponding to the intermediate point; or, in other words, if the dark stripe of the third order described a right line, its distance from the geometrical shadow, at the intermediate point, would be .0599, instead of .0866, as it was found in the experiment: and the difference, .0267, is about half as great again as the interval between the middle points of the second and third bands at the same distance: for this interval was only .01654; and it is evident that the difference .0267 cannot be attributed to any difficulty in ascertaining the exact situation of the point in question, since an error of so great an extent would have implied the passing over not only a bright stripe, but another dark stripe, and half of a second bright one.

I have made many other observations of the same kind, all of which tend to confirm the same singular result. But the experiment now related is sufficient to place beyond doubt the existence of a sensible curvature in the trajectories which represent the passage of the external fringes. [Newton himself had indeed noticed the same curvature in the 9th observation of his third book, and had inferred from it, "that the light which makes the fringes upon the paper is *not the same* light at all distances of the paper from the knives."]

It appears [however] that this remarkable result is very difficult to reconcile with the system of emission: for the most natural manner of explaining the external fringes, in this system, would be to suppose, that the pencil of light which nearly touches the margin of the opaque body undergoes in its neighbourhood alternate dilatations and condensations, which give rise to dark and bright stripes: but then these different bundles of pencils, so condensed and dilated, ought to proceed in a right line, after having passed the body: for if we admit, in the Newtonian theory, that bodies may exert very powerful attractions and repulsions on the molecules of light, it has never been imagined that these forces can extend their action to distances so considerable as the dimensions of these trajectories would require, since they exhibit a sensible curvature for the length of se-

veral yards: and this new hypothesis would introduce a multitude of difficulties still more embarrassing than the one in question.

In all such experiments, the margin of the shadow is so confounded with the fringe of the first order, that it is impossible to determine its limits by the eye only. I employ, for making the shadow, a metallic wire or cylinder, large enough to prevent any confusion from the effect of the light inflected at the opposite side; a point which I ascertain by enlarging its breadth by the addition of a small substance at one part, and finding that the external fringe opposite to it remains unaltered. Then, if I wish to ascertain, for example, the situation of the darkest part of the third order of fringes, with respect to the geometrical shadow, I measure the distance between the two dark stripes of the third order on the opposite sides of the shadow, and subtracting from this the computed breadth of the shadow, one half of the remainder will obviously be the interval required. The diameter of the wire is measured by a gage, or beam compass, which allows me to read off the thousandths of an inch by its vernier, and to estimate less than half of this space with sufficient accuracy: and the instrument itself has sometimes been used instead of a wire, taking care that the two plates, or cocks, should be far enough separated to avoid any confusion from their mutual effects, and then deducting the interval of the opposite fringes from the breadth of the geometrical shadow, in order to obtain the double distance required.

The curvilinear motion of the fringes can only be satisfactorily explained by the mutual influence of the luminous rays, whatever theory we may adopt respecting their nature: from this principle only can we conceive how it happens, that the rays inflected or diffracted, in the neighbourhood of the body, can, without ceasing to be propagated in right lines, give rise to curvilinear trajectories of dark and bright stripes: and it is sufficient for this purpose that the different points, in which they are the most strengthened or weakened by their combination, should be situated in curved lines, and not in straight lines. This would happen, for example,

if the external fringes were derived from the meeting of the direct rays with rays reflected from the edge of the opaque body; for then the points of maximum or minimum of illumination, at different distances from the body, would be situated in hyperbolas, having for their foci the luminous point and the edge of the body, as may easily be inferred from the very simple law of the mutual influence of the rays. It is not, indeed, by the combination of these rays only, that the external fringes are produced, for an infinity of other rays, inflected near the body, concur in producing them; but the trajectories of these are curves of the same nature, and these dark and bright shades are always derived from the mutual action of the rays, without which it would be impossible to understand their curvilinear progress.—Whatever system, therefore, we adopt, it becomes necessary to admit that the rays of light exert this mutual influence; which, indeed, is so fully established by the experiments already related, that we may now consider it as one of the best established principles in optics.

It is very difficult to conceive the existence of a similar phenomenon upon the system of emission, which does not allow us to suppose any connexion between the motions of the several particles of light, without destroying the fundamental hypothesis of the system. We should, at any rate, be obliged to admit, that the mutual action of the rays in such cases is only apparent, and has no real existence; or, in other words, that the phenomenon takes place within the eye only, where the successive impulses of the luminous molecules, on the optic nerve, might be imagined to augment or to diminish the magnitude of the vibrations already commencing, accordingly as they might tend to favour or to check the elementary motions constituting these vibrations: in the same manner as a ringer, when he is raising a bell into its swing, is obliged not only to multiply his efforts on the rope, but to leave between them a proper and regular period, depending on the capability of the bell to perform its vibrations, so that each new impulse may tend to co-operate with the motion already communicated to it.

This ingenious explanation, suggested by Dr. YOUNG himself to the advocates of the system of emission, presents great difficulties when it is followed in its consequences, and is compared with facts. But this is not the place for examining them in detail; the question is more particularly investigated in a Memoir on Diffraction, which is to be printed in the Collection des Savans étrangers. Besides, the necessity of the discussion seems to be superseded by the phenomena of diffraction which are about to be considered, and which appear to be manifestly contradictory to the system of emission.

Dr. YOUNG had supposed, and I had afterwards been of the same opinion, without having been informed of what he had published on the subject, that the external fringes are produced by the meeting of the direct light with the rays reflected on the termination of the opaque body; but, if this had been the case, the edge of a razor, which presents so very small a reflecting surface, should exhibit these fringes much less conspicuously than the back of the razor, which reflects so much more light; while in fact we observe very little difference in the intensity of the fringes thus produced, unless we examine them very near to the razor.

When we transmit the rays proceeding from a luminous point, through an aperture about one-fiftieth of an inch in breadth, and of any convenient length; if the luminous point is not too near to the aperture, we find, as we observe at a greater and greater distance from it, the pencil of rays spread very considerably, and illuminate a space on the card, or form a stripe of light in the focus of the lens, which is much wider than the conical projection of the aperture, which would be formed by right lines drawn from the point in the direction of tangents to the edges of the aperture.

We may consider the edges as very thin, like those of two razors perfectly sharpened, in order to make our reasoning the simpler. Now, if only the rays, which actually pass close to the edges, were subject to any kind of inflection, it is obvious that a very small part of the light transmitted would be spread towards the shadow; and the inflected rays would exhibit but a very feeble light, in the

middle of which we should see, distinctly marked, the bright stripe illuminated by the direct rays: while, in fact, at a proper distance, we observe an illumination nearly uniform, extending over a space much wider than the real projection of the aperture. The dimensions of the aperture are here assumed to be small, in order that the experiment may easily be made in a dark room of fifteen or twenty feet in length; but when the luminous point is at an infinite distance, like a star, we may obtain the same effect with an aperture of any greater magnitude, provided that we observe at a distance sufficiently great.

It follows from these experiments, that the rays of light may be made to deviate from their primitive direction, by the neighbourhood of a screen, not only when they come into contact with the edges of the screen, but even at very sensible distances from these edges.

Let us now consider the consequences of this principle upon the system of emission. If the particles of light are disturbed in the direction of their motions by the influence of the body, in passing at sensible distances from their surface, we must necessarily suppose that this effect is produced by attractive or repulsive forces emanating from the body, and extending to the distances in question, or we must attribute it to little atmospheres, of a similar extent, possessed of different refractive powers from the surrounding medium. But on either of these suppositions it would follow, that the inflection of the rays should vary with the form, the thickness, or the nature of the margin of the aperture, in the experiment which has been last described; while, in fact, the most accurate measurements exhibit no differences whatever arising from these circumstances, and show that the dilatation of the luminous pencils depends merely on the breadth of the aperture. *Hence it may be inferred, that the phenomena of diffraction are incapable of being explained by the system of emission.*

As this objection appears to be of high importance, and decisive of the question, I think it necessary to describe more particularly some of the experiments which establish the principle on which it depends.

I caused a pencil of light to pass between two plates of

steel, placed very near to each other, their vertical edges, which were perfectly rectilinear, being ground to an edge at one part, and rounded at the other, the sharp edge of each being opposite to the rounded edge of the other; the sharp edge being to the right, for example, at the upper part, and to the left at the lower. In this manner, however small the difference might have been in the actions of the two parts on the light, it could not but have been observed at the point of junction of the two corresponding portions of the enlightened space, and more especially at the points of junction of the fringes, which accompanied it, and which would have been interrupted where the sharp and the round edges were abruptly interchanged. But, upon observing these stripes with great attention, I could discover in them nothing like interruption or bending at any part of their length; they were straight and continued, exactly as if the plates had been placed with their similar portions opposed to each other.

Several years before this time, MALUS and BERTHOLLET, in making experiments on diffraction, with plates composed of two parts of different nature, one of ivory, and the other of metal, for example, had ascertained from the situation of the fringes, that the diffractive effects of these different substances were the same; and though the observations of these celebrated persons could not possess all the accuracy of the measurements obtained by the micrometer, in the way which I have described, they were still sufficient to demonstrate, that if the difference of the nature of the substances had some unperceived influence on the direction of the rays, it was still much less than would be expected from the great difference of the refractive and reflective powers of the substances employed, if we supposed the inflection of light to be produced by attractive or repulsive forces acting on the luminous particles. [Dr. Young had long before made some similar observations, in *consequence of* which he asserted, in his Lectures, published in 1807, that "there is some improbability in supposing that bodies of *different forms*, and of various *refractive powers*, should possess an equal force of inflection, *as they appear to do*, in the production of these effects." P. 458.]

I shall mention another experiment, which fully demonstrates that the mass and the nature of the edges of the screen exert no appreciable influence on the deviation of the rays of light.

I covered a plate of glass with a coat of Indian ink, laid on a thin sheet of paper, making together a thickness of $\frac{1}{230}$ of an inch, and having cut two parallel lines through them with the point of a penknife, I removed the strip of blackened paper from the space between them. Having measured the breadth of this space with the micrometer, I formed a similar aperture by means of two cylinders of solid brass, each about half an inch in diameter; and I placed them by the side of the blackened glass, and at the same distance from the luminous point. Observing, then, by means of the micrometer, the pencils of light which had passed through the two apertures, I found the dilatation of the breadth exactly the same in both cases. Now, with respect to the mass of the substances forming the edges of the two apertures, it would be difficult to find them under circumstances more dissimilar: in one case the diffraction was produced by the edges of a simple coat of Indian ink united to a thin piece of paper, since the glass could exert no action in either direction, being continued over the whole aperture: in the other the light was inflected by two cylinders of brass, which presented considerable masses and considerable surfaces to the rays in their neighbourhood.

It is therefore sufficiently proved, that neither the nature of the bodies, nor the mass nor thickness of their terminations, exhibits any sensible influence on the deviation of the rays of light passing near them; and it is equally obvious, that this remarkable circumstance is incapable of being reconciled with the system of emanation.

The theory of undulation, on the contrary, affords us an explanation of it, and even furnishes us with the means of calculating all the phenomena of diffraction; so that the results of the calculation are perfectly confirmed by the observations; as I have shown in the extract of the Memoir on Diffraction, published in the XIth volume of the Annales de Chimie et de Physique.

[To be continued.]

Proceedings of the Royal Society.

The following Papers have been read at the table of the Royal Society since our last report.

February 22nd.

On the Mutual Attraction of the Particles of Magnetic Bodies, and on the Law of Variation of the Magnetic Forces generated by Rotation, by S. H. Christie, Esq.

Correction of an Error in a Paper published in the Philosophical Transactions, entitled "On the Parallax of the Fixed Stars," by J. F. W. Herschel, Esq., F.R.S.

On Attractions apparently Magnetic exhibited during Chemical Combination, by W. L. Henwood, Esq.

March 1st.

On the Submaxillary Gland of the Crocodile, by Thomas Bell, Esq.

March 8th.

On the Chemical Composition of Two Liquids proposed as powerful Disinfectants, by A. B. Granville, M.D., F.R.S.

On the Permeability of Transparent Screens of great tenuity to radiant Heat, by Mr. Ritchie.

March 15th and March 22nd.

Experiments on the Mean Density of the Earth, by Messrs. Whewell and Airey.

March 29th and April 5th.

On the Compounds of Chromium, by Dr. Thomas Thomson.

April 26th.

On the Derangement of certain Transit Instruments by the effect of Temperature, by R. Woodhouse, Esq., F.R.S.

Proceedings of the Royal Institution of Great Britain.

On the evening of *Friday*, the 16th of *March*, Mr. Ainger gave an historical view of the principle and construction of Suspension Bridges. The following is an abstract of his discourse:

Suspension bridges appear to be among the earliest inventions of mankind. They are known to have existed over mountain-clefts and torrents at very remote periods in all parts of the world, especially in Asia; and they are now found, of great magnitude, in countries so little advanced in civilization and science as to prove that they require no considerable exertion either of skill or sagacity. These early bridges consist mostly of ropes manufactured from vegetable fibres, sometimes of wooden bars linked together, and, very seldom, of iron chains. It is said also that, in the East, they use for this purpose the rattan, or common cane, which, when young, is so flexible as to allow of being tied in knots, like a string.

In all these cases the roadway of the bridge has been laid immediately upon the ropes or chains, and has thus partaken of their curvature; differing, in this respect, from the bridges of the last twenty years, in which the roadway, being suspended below the chains, may be made perfectly level. This improvement, combined with the increased and economical manufacture of iron, has given an impulse to constructions of this sort, from which the greatest commercial benefits may be anticipated.

The credit of this application is due to America, whose example has been rapidly followed in this kingdom, and has since extended to the continent of Europe. The most magnificent work of this kind which has yet been executed, is the stupendous bridge over the Menai Strait, erected by Mr. Telford.

The most striking circumstance in these bridges is their great economy as compared with ordinary, or what are called *insistent* bridges. That economy arises from the power of a suspension bridge to vary its curve so as to adapt it to any variation or partial excess in its load, in consequence of which the strength of the chains may be with great precision adjusted to any required strain, and no more: while in insistent bridges the liability of the arch to a fatal derangement of its form by partial or excessive pressure, requires an enormous increase of weight and of strength beyond what is requisite for the mere support of its load, supposing it to be uniformly distributed.

Iron, independently of its cheapness and extensive diffusion, is singularly and admirably adapted for the construction of suspension bridges. When it is considered that the greater part of the weight of these bridges arises from the chains themselves, it is evident that the best material for the purpose is that which has great tenacity with small weight; and thus we find that iron is at the same time the most tenacious and, excepting tin, the lightest of the common metals. A square inch of good iron requires about 28 tons to separate it, and it will not be stretched or otherwise affected with less than half that weight.

Where, however, economy and portability are important objects, rope bridges will be found advantageous, and they have been during the last few years extensively introduced into British India, by Mr. Shakespear, the postmaster-general at Calcutta. They have produced great benefit in facilitating the transport of troops and baggage, as well as of merchandize and the mails. One of these bridges, 160 feet in length, is so light and portable, that it has been several times set up and removed in a few hours. An interesting account of their construction is given, with illustrative engravings, in the 43rd volume of the Transactions of the Society of Arts; from whose repository the Institution was furnished with a beautiful model of one of them transmitted from India by Mr. Shakespear.

At this meeting several valuable specimens in Natural History were presented by Major-general Hardwicke; a variety of curious and instructive models of Pumps, of different constructions, were exhibited; as also several new and splendid Literary Publications.

Friday, March 23rd.

Mr. Reinagle, of the Royal Academy, delivered a discourse on the beautiful forms derivable from the Oval, and its combinations. We hope, in our next number, to be able, through the kindness of Mr. Reinagle, to give an abstract of his lecture, with illustrative wood-cuts.

At this meeting, several specimens of Perforated Metallic Plates were upon the table. These were the work of M. Lariviére, and consisted of plates of various metals perforated with circular apertures, perfectly regular in their size and arrangement, but of different dimensions in different specimens, some of them being of extreme fineness. These holes are formed by punches with flat faces, which, passing through the plates into holes made to receive

them, cut out the pieces to be removed, and leave perfectly regular smooth apertures. The application of plates so perforated, to the purposes of filtration and separation, is daily increasing.

There was also exhibited an Anatomical specimen, which had been preserved for some years in essential oil of turpentine, and upon which had been gradually deposited some very beautiful groups of colourless transparent crystals. Some of these being examined, were found to be fusible, volatile, combustible with a bright carbonaceous flame, burning entirely away, and appeared to consist of hydrogen and carbon, having been formed apparently from a new arrangement of the elements of the oil of turpentine dependant upon time.

The application of a Steam-Engine to the processes of an iron-work, was experimentally illustrated by Mr. Webster, whose models of the different forms of the steam-engine are truly excellent.

Friday, March 30th.

A general view of the Animal Economy, particularly illustrated by a History of the Circulation in man, and other animals, was given in the Theatre, by Dr. Pattison.

A large Meteoric Stone, from the district of Agra, in the East Indies, was placed upon the library table. It belonged to Graham Wilmore, Esq., weighed 14 lb. 1 oz., and was accompanied by a letter and an account of its fall, written in the Persian language, the following translation of which was supplied by Dr. Wilkins:—

Translation of an Account in the Persian Language, of the falling of an Aërolite, in the night of the 7th August, A.D. 1822, *near the Village of* Kadonah, *in the District of* Agra, *East Indies.*

From the perusal of an address from the *Dárógha*, or Superintendant, of *Kosee*, belonging to the district of Agra, dated the 14th of August, A.D. 1822; and another address, dated the 18th September, A.D. 1822, in answer to a written order; it was discovered that in the middle of the night, between the 7th and 8th of August, A.D. 1822, (when all the people of the village of *Kádónah*, belonging to the aforesaid police station (*Kosee*) were asleep in their own houses, except the watchman *Sabalá*, who was employed in the fields of the village, and the zemeendars *Oo-day Sing*, *Sahee Ram*, and *Hargóband;* also the sweeper *Báchá*, who were awake in their own houses,); reports were heard (as it were) of muskets and cannon towards the west, and a noise as the violence of the

wind, and the like. The few inhabitants of the vicinity who were awake, were alike sensible of the reports and noises; but *Sabalá*, the watchman, who was in the fields, besides the aforesaid noises, heard the explosive sound of the falling of some heavy substance on the ground, on that side where are situated the houses of the sweepers, so that he went about on all sides in search; but, in the night, nothing came to his sight. At length, in the morning, when it was daylight, he was again moving about, in order to discover what it was; when on that side, at the distance of about three fields from the cultivated land, he found that a stone, of the dimensions of a span, had fallen down into the ground among the trees of the common; that a little smoke was rising from it, and that it was somewhat warm: also, that the colour of it was just what it now is. The light made it evident that this stone had the preceding night fallen from the atmosphere, and had occasioned the sudden noise. It was also discovered that there had been a hole, of the measure of one span, made in the ground, of which, in consequence of the violence of the rain, not a vestige remained when the superintendent sent the stone to the Court-house. Its colour appears of a red or rusty black, like that of iron. On one side a little has been broken off, from which the stone appears to be a concrete mass.

Mr. Ritchie produced one of his simple but delicate Balances.—See *Quarterly Journal of Science*, xxii. 181.

Specimens of Pemmican, prepared by order of Government for Capt. Parry's voyage, were laid upon the tables by Mr. Marshall.

Experiment on the Reflection of Light.—The quantity of light reflected by bodies is known to increase as the angle which the incident ray makes with the reflecting surface is diminished. This was strikingly illustrated by a plane surface of plaster of Paris: used as a mirror, no image of a luminous or strongly-illuminated body could be seen in it when the rays impinged directly upon the surface, or at considerable angles; but when the plaster was so held that the rays made but a small angle with its surface, then a clear and distinct reflected image of the object was immediately observed. It was stated that even writing-paper, when well stretched and brought into the proper position, could thus be made to act as a reflector.

Several curious MSS. and antient books were placed upon the table.

Friday, April 6th.

Mr. Webster made some observations, in the Lecture-Room, on the effect of the Wind in impelling Sails, &c.

In the Library was a Skeleton of the Ourang Outang of Borneo, presented to the Museum by Major-general Hardwicke; a Patagonian Penguin, presented by Mr. Shipley; the Tooth of a Spermaceti Whale, presented by Mr. Marshall; and specimens of Paper made from various substances. Books were presented to the Library by Sir Thomas Phillips, Bart., John Ranking, George P. Scroope, and T. Winn, Esquires.

The Meetings were then adjourned to the 27th April.

Friday, April 27th.

In the Theatre some further observations on Egyptian Mummies, and on the Art of Embalming, were addressed to the Members by Dr. Granville. On this occasion, Dr. Granville recapitulated the principal heads of his former discourse; and added a variety of new and interesting illustrations in proof of the statements contained in his Essay upon the subject, printed in the Philosophical Transactions.

In the Library was a part of the bottom of a ship, consisting of a piece of four-inch plank pierced through by a sword-fish, the tooth having been broken by the force of the blow, and about 12 inches in length of it remaining in the wood. This specimen was presented to the Museum by Major-general Hardwicke. A poisoned arrow, from the Celebes, such as those which the inhabitants project by blowing them through tubes; and some specimens of the Mantis, being presents to the Museum, were placed upon the table, upon which were also many literary curiosities.

Friday, May 4th.

Mr. Faraday gave an account of the chemical action of Chlorine and its compounds, when used as disinfectants.—It was observed that the power possessed by chlorine in its free state, either as gas or in solution, to destroy vegetable colours, putrid effluvia, and infectious vapours had been long known; and the similar powers of some of its compounds recognised: but that fresh and deserved attention had been again excited towards the subject, by the proposal of M. Labarraque to use the compounds of this element with lime and with carbonated soda, in numerous cases of dis-

infection, and by the proofs he had brought forward of their very efficacious nature. The application of these compounds, and their medical uses, having been spoken of by Mr. Alcock, on February 2nd*, Mr. Faraday confined himself to considerations purely chemical. Free chlorine acts upon putrid or infectious miasmata, sometimes by the abstraction of hydrogen, with which it forms muriatic acid; sometimes by combining with and forming triple compounds with the carbon and hydrogen of the vapours; and sometimes, perhaps, by evolving oxygen, which, in its nascent state, acts upon the effluvia: but in all cases it acts chemically, and converts that which is noxious into inoffensive substances; and this t does by mere contact at common or even very low temperatures.

Free chlorine is often inconveniently applicable, because it is itself offensive and injurious to the lungs; but, when combined with hydrated lime, or with a carbonated alkali, it forms compounds which may be obtained in the solid state, or sometimes more conveniently in a state of solution, and in which, although none of the offensive qualities of the chlorine remain, its power of destroying odorous and infectious matter is still singularly eminent. The first of them is usually known by the name of chloride of lime, or bleaching powder, and is prepared in enormous quantities in this and other countries, for the use of the bleacher. In it the chlorine is held by so weak an affinity, that even carbonic acid evolves it, and it is by such an action that the powder, or its solution, acts as a disinfectant upon the air to which it is exposed; for, according to the good experiments of M. Gaultier de Claubryt, the carbonic acid in the air first evolves a small portion of chlorine, and then the free chlorine acts upon the putrid or infectious matter.

The compound of chlorine with carbonate of soda, which has been distinguished by several very unphilosophical names, is not so well understood. Mr. Faraday inclined to think it the result of a simple union between the two substances, unattended by any decomposition of the alkaline carbonate. He stated, as the result of his own experience, that no carbonic acid gas was given out when prepared according to M. Labarraque's prescription; and as the result of Mr. Phillips's experience, that a peculiar crystalline mass was obtained by evaporating the solution to dryness, which, when dissolved, although it possessed no bleaching power, acquired that property by passing a current of carbonic acid gas through it. He also mentioned an experiment made by Mr. Phillips, at which he was present, in which a portion of the compound which had

* See page 211 of this volume. † See page 237.

been evaporated to dryness and exposed to air for some time, when re-dissolved, and converted by nitric acid into a nitrate, and then tested by nitrate of silver, gave so small a quantity of chloride of silver, as evidently to show that the chlorine had never so far acted upon the alkali as to produce any sensible portion of chloride or chlorate, but had been almost entirely set free again as uncombined chlorine.

The necessity of attending very nicely to the preparation of the substances when they were intended for medical uses, was insisted upon, according to M. Labarraque's directions; but when required for common domestic uses, which must daily become more numerous, the ordinary bleaching powder, or a common soda preparation, were recommended as being quite as effectual and more economical.

Later in the evening, the Mummy of a Cat, presented to the Museum by Mr. Leverton, was opened upon the lecture-table by Dr. Granville, who directed the attention of the audience to the manner in which the bandages were applied and the limbs folded, and to other interesting points which arose during the examination.

In the Library were three Kaleidophones, an instrument, the invention of Mr. Wheatstone, for a description of which, see page 344 of the present number.

On the tables were numerous specimens of antient and modern literary works.

Friday, May 11*th.*

On this evening, Mr. Brockenden offered some remarks upon the Tenacity of Metals, and a new mode of Wire-drawing. He, some years since, patented an invention for drawing fine wire through holes pierced in gems, which, for wire of a valuable material, has been completely successful. Pierced gems are now generally used by the manufacturers of gold, silver, and gilt wire, at Lyons in France, and in England. The advantages of using the holes made in gems, instead of those formerly pierced in metal-plates, are numerous and important. From the perfect lustre which is given to the hole by the diamond-powder used in polishing and finishing, it never removes the gold from the silver and copper-gilt wires, but imparts to the metal a peculiar brilliancy. The durability of the holes is so great, that a piece of wire, 800 miles in length, has been drawn through a hole pierced in a ruby $\frac{1}{300}$ of an inch diameter, without any measurable variation in the size of the ends.

The maximum quantity of such wire drawn through a metal-plate has not been two miles in length, and there it has been found necessary to break the wire and begin with a new hole, as the old one was worn too large, and the increased diameter of the wire occasioned a great loss of valuable material. Holes have been pierced as fine as the $\frac{1}{1000}$th of an inch in ruby and sapphire, and the gilt wire so obtained, and afterwards flattened, has so attenuated the gold, that its calculable thickness was less than the $\frac{1}{100000000}$th of an inch; a platina wire being placed in a silver wire of 90 times its diameter, and the latter reduced to $\frac{1}{1000}$th of an inch, the former was reduced to the *undetectable* thickness of $\frac{1}{10000000}$th of an inch. Mr. B. stated, that he had employed various stones and gems with as various success—agate, jade, heliotrope, chrysoberyl, ruby, sapphire, and diamond. The chrysoberyl, ruby, and sapphire were the best: the first, for its toughness and hardness; the latter, for hardness principally. The diamond failed from the difficulty of polishing the hole*.

The invention was unsuccessful for iron-wire, because the material was found to be too intractable, and stones of sufficient magnitude could not be procured at a moderate price; but if this could have been effected, the value of the invention, as applied to the manufacture of wire for carding-machines, would have been very great; because wire perfectly cylindrical and equal is a desideratum. It would, also, have been of great value for musical wire;—not that pianoforte-makers and tuners would have been sensible of its excellence, as their present test of the fitness of the wire which they use is the *twisting* which it will bear—a quality found in high perfection in *tin wire*, and least of all in steel.

Mr. B. adverted to the vague and confused tables of *ductility* and tenacity usually found in works upon the qualities of metals. Ductility is a term usually employed to express the quality of the metal upon which forces act to reduce it to wire; but if this mean in the way wire is usually made—that is, drawn through a hole—it includes two qualities: the tenacity of the wire drawn, which must exceed the resistance in the whole of that portion which is to be drawn; and the hardness or softness of the metal which is subjected to the reduction. Thus, the ductility of steel and of tin would approximate closely; for the softness of tin is considerable, but its tenacity very little; whilst the hardness and intractibility of steel is so great, that it exceeds its high quality of tenacity, and renders the difficulty nearly equal of drawing steel and tin. But

* The mode of making these holes was also stated by Mr. B., and will be made the subject of a future notice in this work.

if its measure of ductility were to be determined by another mode of forming the wire—extending it between half-round grooves—the ductility of tin, or its reduction to wire by that process, would be at least twenty times more easy than that of steel.

There are many other interesting and uninvestigated qualities in metals, such as twisting, flexion, &c. A tin wire may be twisted twenty times round without breaking, when steel would snap before one revolution. Yet the tenacity of steel must be more than fifty times exceeding that of tin.

Mr. B. exhibited some new instruments for measuring minute differences in the diameters to the $\frac{1}{30000}$th of an inch; and also for determining comparative tenacity, flexion, and elasticity.

In the Library was an engraved copy of an autograph of Oliver Cromwell, the property of Mr. Frost.

Friday, May 18*th, and May* 25*th.*

These evenings were occupied with an account of the recent improvements in ship-building, by Mr. Holdsworth; of which we shall give the details in our next Number.

Friday, June 1*st.*

Mr. Turrell delivered a discourse on the methods of *splitting*, *cutting*, and *polishing* the Diamond; he also entered into a description of the application of that substance to effect many curious and useful purposes in various manufactures practised in this country. It was shown that, although the diamond is the hardest of all known substances, yet it may be split by a steel tool, provided a blow be applied; but this requires a perfect knowledge of the structure, because it will only yield to such means in certain directions. This circumstance prevents the workman from forming facettes or planes generally, by the process of splitting; he is therefore obliged to resort to the process of abrasion, which is technically called *cutting*. The process of cutting is effected by fixing the diamond to be cut on the end of a stick, or handle, in a small ball of cement, that part which is to be reduced being left to project. Another diamond is also fixed in a similar manner, and the two stones being rubbed against each other with considerable force, they are mutually abraded, flat surfaces, or facettes, being thereby produced. Other facettes are formed by shifting the diamonds into fresh positions in the cement, and when a sufficient number are produced they are fit for polishing. The stones, when cut, are fixed for polishing by embedding them in soft solder, con-

tained in a small copper cup, the part, or facette, to be polished being left to protrude.

A flat circular plate of cast-iron is then charged with the powder produced during the abrasion of the diamonds; and by this means a tool is formed that is capable of producing the exquisite lustre so much admired on a finely-polished gem. Those diamonds that are unfit for working, on account of the imperfection of their lustre or colour, are sold for various purposes under the technical name of Bort. Stones of this kind are frequently broken in a steel mortar by repeated blows, until they are reduced to a fine powder, which is used to charge metal plates, of various kinds, for the use of jewellers, lapidaries, and others. Bort, in this state of preparation, is incapable of polishing any gems; but it is used to produce flat surfaces on rubies and other precious stones.

It was also shown that fine drills are made of small splinters of bort, which are used for drilling small holes in rubies and other hard stones, for the use of watch-jewellers, gold and silver wire-drawers, and others who require very fine holes drilled in such substances. These drills are also used to pierce holes in china, where rivets are to be inserted; also for piercing holes in artificial enamel teeth, or any vitreous substances, however hard.

A paper, published by Dr. Wollaston in the Philosophical Transactions of the Royal Society, on the nature and properties of the cutting diamond, was referred to as fully explaining the requisite properties in the natural diamond to suit it to that purpose. In this it was clearly shown, that it is only in the natural state the diamond can be depended upon to cut or divide plates or panes of glass; and even then a particular crystallization is absolutely requisite.

These observations were illustrated by various diamonds, which proved the truth of Dr. Wollaston's remarks. The Lecture concluded by exhibiting some beautiful specimens of steel, ornamented in a most curious manner by Mr. John Barton, of the Royal Mint. The most beautiful display of prismatic colours were seen, produced by divisions 2000 in an inch, the lines being made with a natural diamond. It would be impossible to convey the smallest idea of the exquisite beauty and play of colours that are produced by these means. Several other specimens of dividing were laid before the company, tending to elucidate the value of the diamond when applied to effect certain purposes in the arts.

Friday, June 8th.

Some observations on the recently proposed improvement of the

Steam-Engine, and on the means of obtaining motive power from the Gases, were given in the Lecture-room by Mr. Millington.

In the Library were specimens of an Ichneumon, presented to the Museum by Mr. Barclay; and of the Horns of the Klippspringer Antelope, presented by Mr. Colebrooke. Literary works as usual.

Friday, June 15th.

Mr. Faraday gave an account of the progress and present state of the Thames Tunnel. It was illustrated by many fine drawings from the office of Mr. Brunel; by such parts of the apparatus used as was sufficiently portable; by models, and by specimens of the earth raised from different parts, and of the articles which had at various times made their way through from the river into the shield. After describing generally the sinking of the brick tower, which constitutes the larger part of the present shaft; the particular means employed for advancing the tunnel horizontally, i. e. the construction, nature, and use of the shield, were dwelt upon and explained. That being done, some of the extraordinary circumstances against which the shield had to contend, and over which it successfully prevailed, were described; as, for instance, the occurrence of a pot-hole of gravel, the nature of the moistened silt, the rapid entrance of water, and the runs of earth. A particular history of the late entrance of the water in such manner as to fill the tunnel was then given, with the modes adopted in examining the hole that had been formed, by means of the diving-bell; in filling it with bags of clay, and covering it with grouting; and the description was continued to the formation of the second hole, the satisfactory results that were obtained upon the examination of that and the former, the means adopted for the purpose of stopping it, and the progress made in proving them. The subject was concluded by general remarks upon the abundant reason there was, from the efficacy of the means, from the good ground in advance, and from the known and tried talents of Mr. Brunel, to expect a successful result to this bold and arduous undertaking.

In the Library were two Guanches, from Teneriffe, belonging to Mr. Brettell; a complete set of specimens of Rock Salt, presented to the Museum by Mr. Leigh; new Transactions of various Societies, and new literary works.

The Meetings were then adjourned to the next season.

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *On the Compression of Gases*, by M. Despretz.—Sulphurous and sulphuretted hydrogen, cyanogen, and ammoniacal gases, are not subject to the law of Mariotte. The difference becomes evident under a pressure of two atmospheres, as may be seen by the following table:—

Air.	Ammoniacal Gas.
1.819	1.850
2.582	2.663
3.863	4.132

The gases were perfectly dry, and free from extraneous substances. The graduated tubes containing them were surrounded by water, so that the heat liberated could produce no effect. Hydrogen accorded with air up to 15 atmospheres, and it was only under 20 atmospheres that the former was decidedly in advance of the latter: on repeating the experiment, the tube burst at 28 atmospheres.

These experiments show that the gases susceptible of being liquefied, indicate a pressure, which appears to be greater than that of the air, as the pressure is greater. It is probable that air itself is not subject to the law of Mariotte at pressures but little increased, but that can only be decided by measuring the pressure by a column of mercury, and correcting the latter for the compression of the metal.

In a second communication, the following is described as the method employed by M. Despretz. Two graduated tubes, containing one air, and the other the gas to be compared, were introduced into a large tube containing water, each gas tube dipping into a reservoir of mercury. The gases were perfectly dry; a syringe was attached to the mouth of the large glass tube; by depressing the piston the water was compressed, and, consequently, the gas and air in the tube. In this way, a uniform force was applied; but as the tubes were of the same diameter, and the gas stood in them at the same height, it ought to rise equally in them, in consequence of the pressure, if, at least, the gases were equally compressible. But this did not happen, and, except hydrogen, all the gases were compressed more rapidly than air. To obtain the value of the difference, Mariotte's law was assumed as correct, and the elastic force of each gas calculated from the difference of volume which occurred. It was in this way that the numbers above were obtained.

M. Despretz is engaged in constructing an apparatus by which the different gases may be compressed by a column of mercury, and from which more accurate and advantageous results may be expected.—*Ann. de Chimie*, xxxiv., 335, 443.

2. *On a New Optical Property of Dichroite.*—This mineral, already remarkable for the numerous colours which it exhibits when exposed to light, and more particularly to that which has been polarised, is rendered still more so by a new property which M. Marx has discovered in it,—namely, the power of polarising ordinary light. M. Biot first observed this property in transparent tourmaline, and afterwards used two plates of this substance, cut parallel to the axis of the crystals, and placed one upon the other at right angles, in his investigations of the optical properties of other minerals. Plates of dichroite may be used for the same purpose: those specimens of the mineral which come from Bodenmais are very proper for such uses. According to M. Marx, the curious fact occurs, that the plates may be cut from the crystals in a direction either parallel or perpendicular to the axis. M. Marx thinks that the dichroite does not belong to the rhomboidal system, and also that it possesses two optical axes, the angle formed by them being divided by the axes of crystallization.—*Zeitzchrift fur Mineral*, 1826.

3. *Velocity of Cannon Balls.*—Lieutenant Helwig, of Prussia, has invented a process for measuring the time occupied by a ball or bullet in passing through a certain space. His process consists in making the ball liberate the works of a time-keeper at the moment when it quits the mouth of the piece, and in making it also stop the time-keeper at the moment when it strikes an obstacle. The numerous experiments which he has made already, offer interesting results. He finds, for instance, that a light body, of the same calibre with the bullet, moves, at the commencement, with much greater velocity than the latter, equal charges being used: he finds also, that small bodies move more promptly, a circumstance which causes a considerable deviation of the ball, when there is sand or any light body within the piece used.—*Bul. Univ.* H. iv. p. 119.

4. *Security from Fire in Theatres.*—At the time of re-constructing the theatre, l'Odéon, after its destruction by fire, it became a question whether a metallic screen should not be interposed at the proscenium between the public part and the stage, for the purpose of preventing the rapid communication of the combustion of the parts behind the scene, or upon the stage, to the body of the theatre. M. D'Arcet strongly opposed the use of an impermeable screen, but recommended one of wire, and founded his opinion on the following fact, which, if generally known, might tend to give a degree of self-possession to the public, in a theatre, should they ever be called upon to act in such a critical situation. "I had occasion, accurately, to observe the first destruction of l'Odéon, by fire, in the year 1799. I then remarked that the fire spread rapidly behind the scenes, but without any smoke appearing in the body of the theatre; and I was able to remain, for a long time, in one of the second tier of boxes on the left side, without being inconvenienced by the heat,

and having nothing as a protection but the powerful current of air which passed through the theatre, and proceeded to invigorate the combustion of the scenery and wood-work upon the stage. Nearly the whole of that part of the theatre was on fire, but the public portion quite untouched, when a piece of inflamed wood fell on the left side of the centre, rebounded over the balustrade, fell into the orchestra, and then set fire to one of the benches in the pit. The combustion of that part thus commenced, but the current of air towards the stage was so rapid, that the smoke passed nearly horizontally towards it, and the fire made but slow progress in the pit. I witnessed this effect for more than half an hour, and beheld the orchestra, and nearly the whole of the pit in flames, without there being any sensible portion of smoke in the body of the theatre above the first tier of boxes. I was then obliged to work at a pump, and prevented from observing any longer the progress of the fire."

M. D'Arcet then observes that an impermeable metallic screen would, in any such case, have done far more harm than good. Supposing it strong enough to withstand the pressure of the air upon it, it would quickly become red-hot, and from causing the ascent of a current of highly-heated air on its side towards the body of the theatre, would, in every way, tend to inflame that part of the building; it would also confine the air there, and in a few moments make it unfit for respiration. On the contrary, an open metallic screen, with large meshes, would appear to present great advantage; the air would pass freely, and consequently, exert no serious degree of pressure upon it; the screen itself would be preserved cool by the rapid current going through it: inflamed pieces of wood, when falling, would be retained on the side already in flames; the firemen could throw water through it upon the burning places on the opposite side, and none of the inconveniences would be occasioned by it, which so evidently belong to the impermeable screen.

In Paris, the architect is obliged, by law, to construct a thick wall across the theatre, between the audience part and the stage with all behind or belonging to it. The following are the directions which M. D'Arcet gives in case fire occurs in such a theatre, where the stage opening can be shut at pleasure, by a metallic screen, constructed of wire sixteen hundredths of an inch in thickness, and with square intervals about two inches in width. Suppose the fire to commence with the decorations upon the stage, the immediate help which should always be at hand should be applied, and the firemen sent for; all means should be taken to quell the flames, until the hope of extinguishing the fire is gone: the system should then be changed; the ventilations over the body of the theatre are to be closed, the screen lowered, all the doors into the lower part of the body of the theatre opened to admit air; the ventilating flue over the stage must be opened, and the glass of the sky-lights and upper windows of that part broken by stones, or otherwise: a

powerful current of air will thus be established, which entering by the vestibule, will pass into the theatre through the screen, carry the flame and smoke towards the back of the theatre, and out at the upper apertures on that side. That done, every means possible must be taken to throw the inflamed wood-work towards the bottom of the theatre, to prevent the calcination of the large wall. The firemen, in the pit, should water the wires of the metallic screen, and extinguish the flames near to it. Some of them, with long poles, should be ready to thrust any scenery or inflamed wood which may fall against the screen backwards, whilst others should watch the body of the theatre to extinguish any small portions of fire that might accidentally pass the screen. The fire thus confined to the back of the theatre, would be more readily extinguished by the ordinary means; or if the fire happened on the other side of the screen, then a similar system should be pursued, always considering that part in which the fire is strongly established as a furnace, and removing all burning portions as fast as possible from the part to be preserved towards the place where combustion is allowed.—*Bul. Univ.* E. vii. 15.

5. *Macadamised Roads.*—The cost of converting Regent-street, Whitehall, and Palace-yard, into broken-stone roads, has been £6055 8 3

Value of old pavement taken up and broken for that purpose 6787 7 0

£12,842 15 3

Parliamentary papers. M. Post. May 19th.

II. Chemical Science.

1. *Determination of the Electro-conducting Power of Metals.*—The following is a very simple and exact process by which M. Arago ascertains the conducting power of different bodies for the electric fluid, when the latter is accumulated upon large surfaces, in a state of great tension. Suppose a conducting wire to be continued from the external coating of a battery, in a rectilineal direction, for a certain length, and then ramifying in a certain number of branches of the same metal, all equal in diameter, form, and length, and reuniting in one common point. Steel needles are to be placed transversely across the straight part of each wire before and after the ramification, and the electric discharge is to be passed through the system. It will pass undivided through the first conductor, but be separated in the different branches into equal proportions. The magnetization of the needles placed on the first wire will be the measure of the effect produced by the whole quantity of electricity; the magnetization of those placed on the ramified wires will measure the effect produced by a certain fraction of that quantity; as the third, if there be three branches; or the tenth,

if there be ten. A scale will be thus formed of magnetic intensities, corresponding to a fraction of any given discharge. If afterwards, instead of using different branches of the same metal, similar wires of different metals be used, and a second discharge be passed through this new system, equal to one of known effect, it will be divided unequally between the wires, and the steel needles placed transversely across them will indicate by their degree of magnetization, whether a particular metal has transmitted a third, another a fourth, another a tenth, or any other proportion of the whole quantity of electricity discharged.—*Ann. de Chimie*, xxxiv. p. 7.

2. *On Lightning Rods and Compasses, by Dr. Fischer.*—Dr. Fischer states, that when iron is magnetised it loses much of its conducting power for ordinary electricity; and concludes from his observations, that iron, which is known to become magnetic by even feeble electric explosions, is a very improper metal for the construction of lightning-rods. He relates an instance in which a rod of this metal, placed as a lightning-conductor upon a powder-magazine, had entirely failed on several occasions in preventing the explosion of lightning close by its side. Upon examining this rod, it was found to have attractive and repulsive magnetic properties.

M. Fischer consequently recommends copper as the metal to be used, and advises that the end be gilt or made to terminate in a gold point, rather than with platina, because the former is so much superior in conducting power.

The conclusions of M. Fischer with regard to the decrease of conducting power in iron, when magnetised, are in singular contrast to the assertions of Mr. Abraham, who states that the conducting property is wonderfully increased by making iron magnetic, and recommends that all lightning-rods be of magnetised iron. We doubt whether any effect is produced either way.

Mr. Fischer endeavours to prevent the disturbance of the magnetic needle from neighbouring masses of iron, by placing it in a bowl of that metal of equal thickness throughout. He states, that the needle points north and south, and is not effected by the approximation of iron. The means are the same, generally, as those practised some years ago in this country by Mr. Jennings, who surrounded his needles with a strong ring of iron. The effect is also stated to be the same.—*Bull. Univ.* E. vii. 93.

Mr. Abraham used two cast-steel rods, properly hardened and tempered, each three feet in length and half an inch in diameter; one end of each bar was hammered to a fine point, and one bar was rendered magnetic; then a brass ball was placed two inches off from the prime conductor of a machine, and the points of the unmagnetised and magnetised bars brought alternately towards it; the magnetic bar (it is said) prevented the passage of sparks between the conductor and ball when it was twelve inches off, whilst the other required to be brought to within nine inches.—*Phil. Mag.* N. S. i. p. 267.

3. *On the transference of Ponderable Matter in Electric Discharges, by* M. Fusinieri.—This philosopher has made numerous experiments on this subject, but we think it sufficient to quote the following only. Two large Leyden jars were charged, one of which had a ball of silver, connected with its knob by a silver wire; a polished dish of copper, nine centimetres in diameter and two-thirds of a millimetre in thickness, was then held against the ball of the discharging rod, and the discharge made from the silver ball, through the plate to the brass ball, with which it was in contact. The distance was six or eight lines, such was the length of spark, and the following were the results:—1. A red stain (oxide of copper), with a greenish portion (oxide of silver), and white spots, (silver), was formed on the face of the disc, opposite the silver ball. 2. A stain, more or less green, existed on the other side of the plate, where it was in contact with the brass ball. 3. The brass ball had a white stain, at the point of contact. 4. Finally, the ball of silver had traces of oxide of copper. Besides these effects, the copper and brass were roughened at the points of discharge.

In another experiment, a ball of gold and a disc and discharger of silver were used with the former two jars. The disc was placed in contact with the ball of silver; a gold stain, a centimetre in diameter, was obtained on the external face of the disc, although the electric spark had to pass a distance of from two to four centimetres from the gold to the silver. This beautiful stain was like a very thin film of gold; in the course of some minutes, it became more rare, and totally disappeared in the course of some days,—an effect which sufficiently indicates that the gold had been reduced to a volatile state. There was also a stain of gold on the ball of silver, and a stain of silver on the ball of gold.

Another experiment was made: after having covered one of the faces of the silver-plate with wax, and made a hole in the wax, $1\frac{1}{2}$ millimetres in diameter; the face not covered received a stain of volatile gold, as in a former experiment; but on the face covered with wax, there was a layer of gold of more thickness and solidity, as it did not disappear by spontaneous evaporation.

It is supposed, that these experiments prove the transportation of ponderable matter by the electric spark. The subject being reduced to such a state of division as to become volatile. After this it is supposed, that the light of the spark is due to the presence of molecules of ponderable matter detached by the electricity from the hardest bodies. The diversity of colour in the spark is in the same way supposed to be readily explicable. The light observed in vacuo between the poles of the voltaic light are due, without doubt, it is said, to solid particles carried by the current. The smell occasioned by the electric sparks of our machines, and by lightning, is considered as the odour of the transported matter.—*Gior. di Fisica*, 1825, p. 450.

4. *New Phenomena produced by Vapour, observed by* M. Clement

Désormes.—When steam under high-pressure is allowed to escape in a roaring, violent jet, from rather a large orifice pierced in a plate, and a flat disc is opposed to it at a moderate distance from the aperture, the latter is powerfully forced away; but if it be pressed still more closely, as if to shut the aperture, although the vapour issue on all sides, like an artificial fire-work, and presses more on the disc than before, it may nevertheless be left free: not only will it not be raised, but if the issue for the vapour be turned towards the earth, and the disc consequently tend to fall, as well by its own weight as by the pressure of the vapour, still it will not descend. The same result is obtained if the experiment be made with the current of air from a strong blowing machine, belonging to an iron blast furnace.

Another fact, also very curious, though well known, is that a jet of high-pressure vapour, issuing from a very hot boiler, will appear as a refreshing wind, compared to another jet of vapour coming from a source much less elevated in temperature, and of less than one-twentieth the pressure of the former.

From the first experiment, M. Clement concludes that the common safety-valves, which are real discs placed upon apertures with flat edges, involve a danger inherent in their form. In fact, no sooner are they raised, to permit the escape of a thin film of vapour, than there immediately results the impossibility of rising any further; and if the production of vapour is too considerable for the small aperture which can exist, and for the strength of the boiler, an explosion happens, notwithstanding that the safety-valve is open. This, in fact, is what has actually occurred sometimes, although it has appeared incredible.

M. Clement attributes these effects to the vacuum which is formed in the jet of vapour, in consequence of the extreme velocity of its molecules, and to the conical form of the kind of tube produced by the two approximated plates between which the vapour is forced to pass. It dilates near the edges, in consequence of its momentum and elasticity far below the pressure of the atmosphere, and the latter acts with sufficient force on the moveable disc to resist the vapour.

The remedy to this danger may be found, without doubt, in a good proportion between the orifices and their edges; the former should be large, and the latter small. The addition of a conical tube to safety-valves will diminish the effect of the atmospheric pressure, and of the weight with which they are charged.—*Bull. Univ.* E. vii. p. 41.

In the next number of the *Bulletin Universelle* the following explanation of this phenomenon by M. Clement is given. When a jet of vapour moving with great velocity does not displace a moveable plate put over the orifice by which it escapes, it is because a partial vacuum exists between the fixed plate and the mobile disc, where a tension less than that of the atmosphere exists

throughout a space sufficiently large to permit the pressure of the atmosphere to be stronger than that of the vapour. M. Clement has demonstrated by several experiments that this partial vacuum really exists in the space which serves as an issue for the vapour, just as occurs in the conical tubes which are applied to orifices through which liquids are flowing. He has shown, that the interval which separates the two plates between which the vapour escapes is a true conoide. In fact, this interval is formed of concentric rings, of which the surface augments in proportion to the diameters; and, consequently, the elastic fluid as it passes from the smaller to the larger ring has successively filled the spaces in which it has assumed a decreasing density, until beneath that which corresponds to atmospheric pressure, and this in consequence of the active force with which each molecule is animated when it escapes from its first pressure.

M. Clement finds that the theory which Daniel Bernouilli has given for the effect of conical or cylindrical jets applies perfectly to these curious phenomena with vapour, so that the effects were explained, as it were, before they were observed. The great degree of cold produced by compressed vapour at the moment of its expansion in the air, is also an effect of the active force which it possesses at the moment of its issuing forth, which enables it to acquire a very low tension. The jet is then formed of vapour, of the force of one-half or one-fourth of an atmosphere, and which consequently possesses only the corresponding temperature, *i.e.* 60 or 50 degrees C. This temperature is still further reduced by the quantity of air which the vacuum draws to it, and thus a wind rather cold than hot is occasioned.

M. Clement proposes some alterations in safety-valves, and considers the addition of a weak tube to steam-boilers as a perfect security. He recommends it to be of considerable diameter, and capable of supplying when burst a large vent for the vapour, so as to be sufficient to allow of the sudden productions of vapour which sometimes take place, and to which probably numerous accidents are due.—E. vii. p. 104.

5. *Transference of Heat by change of capacity in Gas.*—Many of the copper vessels in which gas is compressed at the Portable Gas-works are cylinders, from two to three feet in length, terminated by hemispherical ends. These are attached at one end to the system of pipes by which the gas is thrown in, and being so fixed, the communication is opened; it frequently happens, that gas previously at the pressure of thirty atmospheres in the pipes and attached recipients, is suddenly allowed to enter these long gas-vessels, at which time a curious effect is observed. That end of the cylinder at which the gas enters becomes very much cooled, whilst, on the contrary, the other end acquires a considerable rise of temperature. This effect is produced by change of capacity in

the gas; for, as it enters the vessel from the parts in which it was previously confined, at a pressure of thirty atmospheres, it suddenly expands, has its capacity for heat increased, falls in temperature, and consequently cools that part of the vessel with which it first comes in contact; but the part which has thus taken heat from the vessel being thrust forward to the further extremity of the cylinder by the successive portions which enter, is there compressed by them, has its capacity diminished, and now gives out that heat, or a part of it, which it had the moment before absorbed; this it communicates to the metal of that part of the gas-vessel in which it is so compressed, and raises its temperature. Thus the heat of temperature is actually taken up by the gas from one end of the cylinder, and conveyed to the other, occasioning the difference of temperature observed. The effect is best observed when, as before stated, the gas, at a pressure of thirty atmospheres, is suddenly let into the vessels: the capacity of the parts is such, that the pressure usually sinks to about ten atmospheres.

6. *On the Change in the Zero of Thermometers.**—M. Arago remarks, relative to the gradual change which takes place in the indications of the thermometer, in consequence of the elevation of the mercury, that it throws much doubt over numerous observations made with that instrument, and imperatively requires some process by which it can be prevented. The process he recommends is one already known to artists, and practised by them with alcoholic thermometers, and consists in leaving air in the upper part of the tube sufficiently compressed to prevent the disengagement of that contained in the liquid, and the precaution he considers as not less necessary, whether the metal really contains the air in solution, or whether it be considered as interposed only between it and the glass.

Deluc has established, by numerous experiments, "that when from any cause the particles of air disseminated in a liquid gather together, they acquire a greater power to separate the particles of the liquid; and that this effect may be produced long before the small collections of air become visible, and even before they are capable of making their way through the fluid." The excess of force spoken of by Deluc, is manifested by an augmentation of volume, and when the fluid is contained in the tube of a thermometer, by the ascent of the zero; the effect is quick and considerable in thermometers, containing alcohol, water or oil, and should be smaller and less rapid in one containing mercury, though ultimately an analogous result will be obtained.

An experiment made by M. Arago is then described, in which the effect of inclosing air in the thermometer is accurately ascertained. Two similar thermometers were constructed at the same

* Quarterly Journal of Science, xiv. 441; xv. 160, 369, 371.

time, one with the air removed, the other containing air at the ordinary pressure of the atmosphere; the graduation of the last remained accurate after several months, whilst the zero of the first rose nearly a half degree. The bulb of the altered thermometer was so thick, that no effect due to atmospheric pressure acting from without inwards could be supposed a circumstance which excludes the explanation of the effect adopted by MM. Bellani and Flaugergues.

M. Arago concludes by observing that it may perhaps be found preferable to boil the mercury so well as to exclude all air from it; but states that from certain experiments made by M. Dulong, and not as yet published, it would appear that the precautions necessary are so minute and numerous, that the instrument-maker will find it a difficult matter to construct a thermometer of that kind that will not change its zero.—*Annales de Chimie*, xxxiii. 423.

7. *On Bromine and certain of its compounds*, by M. Serullas.—According to M. Balard, bromine remained fluid at a temperature so low as 0° Fahr. M. Serullas found that a degree or two below this it suddenly became solid, and was then hard, and so brittle as to break by a blow.

On adding hydriodide of carbon to bromine in a tube, there was immediate action, much heat, a hissing noise, and a bromide of iodine and a fluid hydro-carburet of bromine were formed;—a further instance this, of the displacement of iodine by brome. Water dissolved the bromide of iodine, and the hydro-carburet with a little excess of bromine remained at the bottom of the fluid. It was rendered colourless by a little alkali. If the hydriodide of carbon is in excess, but little hydro-carburet of bromine is produced, but a sub-bromide of iodine.

The pure hydro-bromide of carbon is colourless, heavier than water, of a penetrating ethereal odour, an exceedingly sweet taste, and very volatile. It does not become coloured in the air like the hydriodide of carbon, and when dropped on ignited porcelain it yields white vapours instead of violet. The same compound was formed by placing a drop of bromine in olefiant gas. When this substance is cooled to about 22° Fahr., it becomes solid, and then breaks in the manner of camphor.

M. Serullas formed hydro-bromic ether by putting forty parts of concentrated alcohol into a retort with one part of phosphorus, and afterwards adding, in small portions, seven or eight parts of bromine; upon each addition there was much action upon the phosphorus; heat was evolved, and hydro-bromic and phosphorus acids produced. Then carefully distilling the mixture, and washing the product with water, the hydro-bromic ether was collected at the bottom. It appeared as a colourless, transparent fluid, heavier than water, with a strong and etherial odour, and a sharp taste; it is very volatile, is soluble in alcohol, but is precipitated by water; it suffers no change of colour

Cyanide of Bromine.—Two parts of dry cyanuret of mercury are to be placed in a tubulated retort, or at the bottom of a long tube, and being well cooled in a refrigerating mixture, one part of bromine is to be added; powerful action ensues, with much development of heat, and a bromide of mercury with a cyanide of bromine are formed: the latter crystallizes in long needles about the middle of the tube, the small quantity of vapour of bromine gradually disappearing. A small receiver being then attached to the tube, and well cooled, the cyanide is to be sublimed into it, and will form small colourless, transparent crystals, sometimes acicular, sometimes cubical. This substance is very volatile, and has a penetrating odour; it becomes vapour at 60° Fahr., and crystallises suddenly by cooling. It is soluble in alcohol and water. Caustic alkalies produce a hydro-cyanate and a hydro-bromate.

The cyanide of bromine is excessively deleterious; a grain dissolved in water, and introduced into the œsophagus of a rabbit caused death as instantly as prussic acid would have done. All attempts to decompose bromine by acting upon it in this compound failed.—*Annales de Chimie*, xxxiv. 95.

8. *On Oxide of Iodine and Iodous Acid*, by Signor Sementini.—When Sementini first obtained iodous acid *, it was by heating a mixture of equal parts of iodine and chlorate of potash; but the latter salt not being pure, variable results were obtained. He then mixed iodine with chlorate of potash in a mortar, adding the former until a clear yellow colour was produced. Upon heating this mixture, yellow vapours appeared at first, which condensing on the neck of the retort used, did not redden the tincture of litmus; but continuing the operation, a dense oleaginous fluid was obtained, reddening litmus, which, exposed to air, first absorbed a little moisture, and then was volatilized; having a very pungent, unpleasant and slightly acrid taste, and being decomposed by phosphorus, sodium, and potassium at common temperature, with the evolution of vapours of iodine.

In the operation referred to, it appeared that the first portion of iodine and oxygen combined to form a new compound oxide of iodine; that afterwards the action of the heat caused the combination of a larger proportion of oxygen with the iodine, and produced two new compounds,—the iodous acid, which, being volatile at a moderate temperature, was condensed in the neck of the retort; and iodic acid, which remained combined with the potash of the chlorate decomposed; a small portion of chlorine being at the same time set free, and rendering the last portions of iodous acid impure by its presence.

In consequence of these actions, the following method of preparing iodous acid is preferable to that first published, as supplying

* Quarterly Journal of Science, vol. xvii. p. 381.

it in a purer state, and in greater abundance. Chlorate of potassa is to be placed in a retort, with rather a large neck, and heated until the moisture is dissipated; then, at the time when oxygen is freely evolved from the salt, iodine is to be introduced by means of a metallic spoon as far as the body of the retort. The vapours of iodine which will be occasioned by the heat will immediately disappear, in consequence of their combination with the oxygen of the chlorate, and a strong effervescence will be perceived in the latter, occasioned by each portion of iodine with which it may combine; immediately yellow vapours will be perceived in the neck of the retort, and when they redden litmus paper, and a sufficient quantity of iodous acid has been obtained, the operation is to be finished, and the acid collected, the last portion being removed by a drop or two of water.

All attempts to unite this acid with alkaline bases have failed, the iodine being precipitated; this happens equally with ammonia, potassa, and soda, the attraction between the elements of the acid being so weak as to allow of their complete separation on these occasions.

Oxide of Iodine was discovered in attempts to combine iodine and oxygen directly together. It is to be obtained in the following manner. A copper tube about two palms (20.6 inches) in length, and eight lines in diameter, having one extremity terminated in a screw, and the other by a small aperture about a line in diameter, was fixed upon two supports; the contracted extremity was curved, so as to permit of its introduction into the tubular of a retort, without deranging its convenient inclination. A bladder filled with oxygen gas was attached to the screw. A long spirit-lamp with several wicks was placed beneath the lower part of the tube, so as, when necessary, to raise it to redness. Arrangements were also made for applying a lamp to the bottom of the retort, to elevate its temperature, it having been found that to combine oxygen and iodine, both should be at a high heat.

The tube and the retort being heated to redness, one person then presses the bladder to force heated oxygen into the retort, and another introduces a spoonful of iodine through the neck of the retort until it is brought under the jet of gas. The iodine is quickly converted into violet vapour, which fills the retort, but this soon disappears, and the retort neck becomes lined with a yellow transparent substance, almost solid; which, by the continuance of the operation, becomes more fluid, and flows forward as an oleaginous liquid. If the violet vapours are too abundant, the spoon is to be withdrawn for a little while, the introduction of oxygen being continued; when they have disappeared the iodine is to be re-advanced.

The substance obtained in the commencement of this operation, is oxide of iodine. It is of the consistence of a solid oil, has an acrid, disgusting taste, an odour similar to that of iodous acid, but

weaker. It is slowly dissipated in the air, is very soluble in water and alcohol, producing amber-coloured solutions. If brought, in the solid state, into contact with a dry, combustible substance, it is decomposed, iodine being evolved. Phosphorus and potassium are inflamed by it. It changes the blue litmus infusion to green. If an alkali be poured into its solution, the latter is instantly rendered colourless; whereas if alkali be added to solution of iodous acid, iodine is precipitated. The discoloration is due to the combination of the alkali with the iodine which is taken from the compound,—a further proof of the weakness of the affinity between oxygen and iodine.

If the addition of oxygen be continued after the formation of the oxide of iodine, then iodous acid is formed; and, if the lamp applied to the bottom of the retort be at the same time removed to the neck, the formation of the iodous acid will be much facilitated.

Finally, oxide of iodine is always formed when iodine is heated in close vessels containing air, and air may be used in place of oxygen in the process above described. If iodine be put into a retort, and then a second be sealed hermetically to the first, a small capillary hole being left in the neck; the iodine being heated successively, first in one part and then in another of the apparatus, wherever it assumes the solid form, will gradually change into oxide of iodine.—*Giornale de Fisica*, ix. p. 387.

9. *Quantity of Charcoal obtained from Wood.*—The following table contains the results obtained by M. Karsten from one hundred parts of the shavings of woods which had been carefully dried in the air. Two modes of carbonization were used; the one rapid and commencing with a high temperature, the other slow and gradual. The quantity of ashes was obtained by incineration of the charcoal under a muffle.

	Rapid.		Slow	
	Charcoal.	Ashes.	Charcoal.	Ashes.
Young oak	16.39..	0.15	25.45..	0.15
Old oak	15.80..	0.11	25.60..	0.11
Young beech, *fagus sylvatica*	14.50..	0.375	25.50..	0.375
Old beech	13.75..	0.4	25.75..	0.4
Young hornbeam, *carpinus betulus*	12.80..	0.32	24.90..	0.32
Old hornbeam	13.30..	0.35	26.10..	0.35
Young alder	14.10..	0.35	25.30..	0.35
Old alder	14.90..	0.40	25.25..	0.40
Young birch	12.80..	0.25	24.80..	0.25
Old birch	11.90..	0.30	24.40..	0.30
Young pine, *pinus picea*	14.10..	0.15	25.10..	0.15
Old pine	13.90..	0.15	24.85..	0.15
Young Norwegian pine, *pinus abies*	16.00..	0.225	27.50..	0.225
Od Norwegian pine	15.10..	0.25	24.50..	0.25
Young Scotch fir, *pinus sylvestris*	15.40..	0.12	25.95..	0.12

	Rapid.		Slow.	
	Charcoal.	Ashes.	Charcoal.	Ashes.
Old fir	13.60.	0.15	25.80.	0.15
Lime	12.90.	0.40	24.20.	0.40
Rye straw	13.10.	0.30	24.30.	0.30
Fern	14.25.	2.75	25.20.	2.75
Reed	12.95.	1.70	24.75.	1.70
Old birch wood	12.15		25.10	

Jamieson's Jour., 1827, p. 287.

10. *Preparation of Cyanide of Iodine, by* M. Serullas.—Place a large glass jar with its mouth upon a plate of glass: mix two parts of cyanide of mercury with one part of iodine, and heat the mixture in a small porcelain capsule until the temperature being sufficiently raised, the cyanide begins to form; then quickly place it under the jar, and leave the operation to itself; the production of cyanide of iodine will continue for fifteen or twenty minutes, presenting the appearance of a multitude of snow-white flocculi, of the utmost degree of whiteness and beauty: these will gradually collect on the plate, and may be afterwards secured.—*Annales de Chimie*, xxxiv. p. 100.

11. *On the Detection of Hydrocyanic Acid in the bodies of Animals poisoned by it, by* MM. Lassaigne and Leuret.—The extreme delicacy of the test employed by M. Lassaigne to detect prussic acid in the bodies of poisoned animals, namely, by means of the per-sulphate of iron, or the per-sulphate of copper, has been pointed out at page 397 of vol. xviii. of the former series of this Journal. Being desirous of ascertaining after how long a period the poison might be discovered, MM. Lassaigne and Leuret made experiments in which animals were killed by hydrocyanic acid introduced into their food, or thrown into the stomach in a diluted state, the dose never surpassing more than the equivalent of five or six drops of the pure acid, or less than two drops; the animals were left after death for twenty-four hours in a chamber, and then buried in moist earth a foot and a-half or two feet deep.

The disinterment took place after various periods, from fifteen days to a month, and the stomach and first portions of intestines being separated, were well divided and mixed with pure water, and distilled; a little sulphuric acid was added to the substances in the alembic, and the recipient was cooled by ice and water.

The products were rendered slightly alkaline, and then tested by the per-sulphate of iron or sulphate of copper, and a little excess of muriatic acid was poured upon the precipitates occasioned by these salts. No prussic acid could be discovered in animals, the viscera of which were in a state of complete putrefaction; it was found after intervals of two or three days, but never after a longer period than eight days.

From these experiments it appears, 1. That prussic acid cannot be discovered in animals poisoned by small quantities of it after exposition of these bodies for two or three days to the air. 2. That the disappearance of the poison in the viscera after a longer period than this is due to its decomposition, which is favoured by the contact of the putrescent animal matter. 3. That when it is necessary to examine a body judicially to ascertain the presence of this poison, it is important that it should be done as quickly as possible.—*Jour. de Chimie Med. Dec.*

12. *On the presence of Ammonia in the natural oxide of Iron, by* M. Chevallier.—Austin stated that ammonia was always formed when iron rusted in contact with water and the air. M. Chevallier having taken every precaution to exclude ammonia, heated two ounces of clean iron-turnings in a crucible, and when cold put them with one ounce of water into a flask, having its mouth plunged under mercury. At the end of ten hours reddened litmus paper which had been introduced into the flask was restored to blue, and four days after the water saturated by muriatic acid gave a sensible quantity of sal ammoniac. The same result was obtained upon every repetition of the experiment, thus proving what Austin had advanced.

Having collected various specimens of native oxides of iron, M. Chevallier then heated them in tubes, and obtained ammonia from them all; but being led to the objection that might be urged of its having been produced from animal matter, he proceeded to operate by washing the finely-pulverized ores with boiling water: in this way he always procured traces of ammonia; one hundred and fifty grammes of the red hematite of Spain gave two grammes of muriate of ammonia, which is one seventy-fifth of the whole weight of the substance.—*Annales de Chimie*, xxxiv. 109.

13. *Volcanic Muriate of Ammonia of the Isle of Lanzerole.* Dr. Brandes.—On July 29, 1825, an earthquake occurred in the island; on the 30th much subterranean noise, and on the 31st, at seven o'clock A.M., the earth opened about a league to the west of the capital between Tao and Tia-Agua, and half a league from Mont Francia. Flames came from the aperture, and stones in such quantity, that a high hill was formed in twenty-four hours. The eruption was strongest on the night of the 31st, the whole island being enlightened. On Aug. 1, at ten o'clock A.M., the fire ceased, but much smoke issued, which, by the 2nd, had formed three columns, one black, one white, and a third red, which was at some distance from the others. Many cisterns became dry. On the 4th of August there was still much smoke; on the 22nd, at seven o'clock A.M., the volcano threw out water, which continued to flow for several days. The lava ejected covered a space of half a league in length, and a quarter of a league in width, and was

not properly a current; the lava was heavy or porous, or even pumice-like in various parts, and was covered with muriate of ammonia mingled with a small quantity of arsenic and magnesia, and with two other salts of selenium and iodine.—*Bul. Univ.*, B. x. 45.

14. *On the efflorescent Salts of Volcanic Rocks.*—MM. Bischoff and Noggerath have examined the saline efflorescence of the Terras in the vallies of Brohl and Tormestein, and found them composed of 18.901 of sulphate of potash, 18.273 of muriate of potash, 43.872 of carbonate of potash, and 20.616 of carbonate of soda. Another salt from Brohl gave sulphate of alumina, with a little oxide of iron, magnesia, a little alkali, and muriatic acid. They endeavoured to discover whether these salts existed, previously formed in the terras; they found potash, soda, and muriatic acid in the latter, besides the ordinary earths; the principal part of the alkalies was combined with the earths, the rest with the acids. The formation of the salts is attributed to the presence of water, and more or less heat.

15. *On the Nitrates of Ammonia and Mercury, by* M. Soubeiran.—If ammonia be added gradually to a very weak solution of pure proto-nitrate of mercury, two precipitates are obtained, one of a grey black, and the other white; the latter being most abundant the nearer the precipitation is to its termination. If the precipitate be thrown down and removed in separate portions, the white preparation may be obtained nearly pure; it is insipid, inodorous, insoluble in hot or cold water. It is not attacked by caustic alkalies, or by nitric or sulphuric acid. The substance was analyzed with difficulty, and the result was as follows:—

Protoxide of mercury	92.3	4	proportionals
Nitric acid	5.85	1	do.
Ammonia	1.85	1	do.
	100.00		

The black precipitate always contained portions of the white substance, and when acted upon by nitric acid left the latter undissolved. M. Soubeiran considers it in its purest state as composed of one proportional of acid, and four of protoxide of mercury.

The ammoniaco-mercurial deuto-nitrate may be obtained by adding excess of ammonia to a dilute solution of the deuto-nitrate of mercury. The white precipitate obtained is insoluble in water, but soluble in muriatic acid, from which potash or soda precipitates it; ammonia produces a precipitate in the solution, resoluble in excess of the alkali. Nitric and sulphuric acids partly dissolve it. It is formed of

Deutoxide of mercury	86.4	2	proportionals.
Ammonia	3.27	1	do.
Nitric acid	10.33	1	do.

Bull. Univ. A. tom. vii. p. 210.

16. *Peroxide of Potash from Nitre.*—Mr. Phillips decomposed nitre by heat, and found the last gaseous products nearly pure azote. Upon pouring water into the gun-barrel to remove the potash, he found that oxygen gas was immediately evolved, and in such quantity that an ignited stick was instantly inflamed, and the combustion continued for a considerable period.—*Phil. Mag. N. S.* i. p. 312.

17. *Crystallized Carbonate of Potash.*—Mr. Phillips finds this salt to be composed of

1 atom of dry carbonate of potash . .	70
2 do. water	18
	88

—*Philosophical Mag.* i. 469.

18. *Sulphates of Manganese and Soda.*—M. Geiger has found two double salts in the residue left during the preparation of chlorine. They are composed of sulphuric acid, oxide of manganese, and soda. The sulphate of manganese and sulphate of soda are in equal proportion in both: but one contains five parts of water, and effloresces in dry air; whilst the second contains only two parts, and is not altered in a similar situation: both salts deliquesce in moist air.—*Bull. Univ.* C. x. 117.

19. *Composition of White Copper, or Packfong.*—M. Gersdorf, of Vienna, says, that the proportions of the metals in this alloy should vary according to the uses for which it is destined. When it is to be used to replace silver, it should be composed of 25 parts nickel, 25 zinc, and 50 of copper. An alloy more proper for rolling consists of 25 of nickel, 20 of zinc, and 60 of copper. Cast pieces, such as candlesticks, bells, &c., are to be made of an alloy composed of 20 parts nickel, 20 zinc, 60 copper, to which 3 of lead have been added. An addition of 2 or 2¼ of iron or steel renders the *packfong* much whiter, but at the same time harder and more brittle.—*Annal. für Phys.*

20. *Method of Bronzing Tin, by* M. Verly.—To obtain complete success in bronzing medals of tin, the two following solutions must be employed: the first, which is merely a wash, is composed of 1 part of iron, 1 part of sulphate of copper, and 20 parts, by weight, of distilled water. The second solution (or bronze) is composed of 4 parts of verdigris and 16 parts of white vinegar. The medals should be filed, and well cleaned with a brush, earth, and water; and, being well wiped, should have a portion of the first solution passed lightly over their faces, by means of a brush, and then be wiped; this gives a slight grey tint to the surface, and causes the ready adhesion of the verdigris, &c. The second solution is then to be

rubbed over them by means of a brush, until they have acquired the deep red colour of copper; they are then to be left for an hour to dry, after which they are to be polished with a very soft brush and rouge, or the red oxide of iron in fine powder, the breath being passed over them from time to time to cause the adhesion of the rouge. The polish is to be completed by the brush alone, the medals being passed now and then over the palm of the hand. If the bronze is to resist humidity, it should be covered with a thin film of gold lacquer.

The pieces of metal formed from Darcet's alloy are bronzed by the use of the second solution only, and do not require lacquering for the purpose of protecting them from the action of water.—*Bull. Univ.* F. vii. 88.

21. *Chromate of Silver crystallized.*—Mr. Teschemacher obtained crystals of this salt from the solution left after separating the precipitate occasioned by adding nitrate of silver to chromate of potash. The solution being allowed to evaporate spontaneously, produced the crystals at the end of ten days. The crystals had a strong metallic lustre, and were of a deep red colour by transmitted light, much resembling native red silver. They were insoluble in cold or hot water, and were reduced by heat to metallic silver and chromic oxide.—*Phil. Mag. N. S.* i. 345.

On a former occasion, the formation of artificial crystals of chromate of lead was noticed*; and it is much to be desired that chemists would attend, as Mr. Teschemacher has done, to the production of the crystals of substances usually considered as insoluble, many of which can no doubt be formed by the use of alkaline, acid, or saline solutions.

22. *Sulphate of Quinia.*—According to the account of MM. Pelletier and Caventou, the large quantity of 90,000 ounces of sulphate of quinia was manufactured in 1826 in France; a quantity which could not have been administered to less than 1,444,000 persons.

23. *Adulteration of Sulphate of Quinia with Sugar.*—M. Winkler has met with sulphate of quinia, in commerce, adulterated with sugar. The quantity of adulteration was ascertained by dissolving the salt in water, precipitating the quinia by carbonate of potash, filtering the liquid, evaporating to dryness, and digesting the residue in alcohol. This fluid left the sulphate and carbonate of potash, but dissolved the sugar, and, by evaporation, yielded the latter in its separate state.

M. Winkler has also met with benzoic acid adulterated with sulphate of lime, in crystals.—*Bull. Univ.* C. x. 178.

* Quarterly Journal of Science, xix. 155.

24. *Composition of Vegetable proximate Principles.*—M. Marcet has analysed several vegetable substances by means of oxide of copper, and finds them to have the following composition:—

Roasted starch, or artificial gum—
Carbon.....35.7
Oxygen.....58.1
Hydrogen..... 6.2

Ordinary Starch—Carbon.....43.7
Oxygen.....49.7
Hydrogen..... 6.6

It appears, therefore, that roasted starch contains much more oxygen and less carbon than common starch. The quantity of hydrogen is also increased.

Starch obtained from malt gave—
Carbon.....41.6
Oxygen.....51.8
Hydrogen..... 6.6

Hordein— Carbon.....44.2
Oxygen.....47.6
Hydrogen..... 6.4
Nitrogen 1.8

The parenchyma of the potato gave—
Carbon.....37.4
Oxygen.....58.6
Hydrogen..... 4.0

Gluten— Carbon.....55.7
Oxygen.....22.0
Hydrogen..... 7.8
Nitrogen 14.5

Zimoma did not appear to differ from common gluten.

Yeast— Carbon.....30.5
Oxygen.....57.4
Hydrogen..... 4.5
Nitrogen ..... 7.6—*Geneva Memoires.*

25. *On Legumine, a particular Vegetable Principle, by* M. H Braconnot.—Dry ripe peas, being left for some hours in warm water to soften, were made into a paste in a mortar with water; and being diffused through more of that fluid, were passed through a sieve: the milky fluid produced being allowed to stand, deposited the starch, the legumine being retained in solution apparently by a vegetable acid. Being evaporated, the legumine separated in successive transparent slightly-soluble pellicles. Thus obtained, it was impure, and of a greenish colour; but being washed with boiling alcohol, the green colour was removed, and the legumine obtained perfectly pure and white; it was now as finely divided as starch, and, when dried, appeared semi-transparent.

Diluted vegetable acids dissolve this principle with great facility; the mineral acids, on the contrary, precipitate these solutions, forming slightly acid and nearly insoluble compounds. If, when thus precipitated, heat be applied, the substance disappears; but on cooling the liquor becomes a gelatinous mass, resembling paste, which, by the application of heat, again liquefies. These precipitates, or pure legumine, heated with a little of a vegetable acid, the tartaric for instance, produces a thick mucilaginous liquid, which, being diluted, is scarcely sour to the tongue. Infusion of galls precipitates this solution; alcohol does not alter it. The mineral acids produce precipitates: earthy and metallic salts with mineral acids precipitate it.

The feeblest alkalies and the alkaline earths dissolve legumine, or its insoluble acid compounds. In order to confirm the suspicions which were entertained of the analogy between the legumine and alkaline substances, the insoluble compound of the substance with nitric acid was well washed in boiling alcohol; it was then put into water rendered slightly alkaline by a little ammonia, after which alcohol was added to precipitate the legumine. When well washed, it resembled paste: it retained no ammonia; and yet, when brought into contact with litmus paper reddened by acid, it restored its blue colour. Being spread upon paper, it dried and produced a brilliant varnish: exposed to heat, it liquefied without coagulating. Iodine, mixed with it in water, appeared to dissolve it; a yellow precipitate was produced by heat, unchanged by the action of alcohol or by drying. It was insoluble in boiling water, and produced a deep blue colour with starch. Being exposed in a glass tube to a careful heat raised above 212° Fahr., violet vapours of iodine were produced, and the legumine remained unchanged.—*Ann. de Chimie*, xxxiv. 68.

26. *Substances contained in Essential Oils.*—Many of the essential oils become changed by exposure to air, and an acid is formed in them, which, crystallizing in acicular needles, may be separated, and has been considered as benzoic acid. M. Bizio finds that oil of cassia affords the largest quantity of this substance, a few days only being sufficient to cause its formation. He collected the crystals together and separated them from the adhering oil, either by means of bibulous paper; or else by warming them in a weak solution of carbonate of potash, filtering the solution of the salt formed from the insoluble oil, boiling it with animal charcoal, afterwards decomposing the purified and crystallized salt by muriatic acid, and separating the acid in question by dissolving it in alcohol.

This acid, although it agrees with benzoic acid in some qualities, differs from it in others. It has no odour or taste, is heavier than water, fuzes and sublimes by means of heat, condensing in brilliant crystals; burns in the air with flame; dissolves in very small quantity in cold water, but far more readily in boiling water, the acid crystallizing as the solution cools. It dissolves in

alcohol both at common and high temperatures. Cold nitric acid has but little action upon it: hot nitric acid acts powerfully, evolving much nitric oxide; and the substance which separates when the solution is cold, no longer has the properties of the original acid. Sulphuric acid acts still more powerfully, decomposing the substance even when cold, and when heated acting rapidly upon it, producing a black solution, and changing the properties of the body altogether. The action of these acids offers distinctions between this substance and benzoic acid.

If the essential oils be exposed for a longer time to the air, there occurs, at a later period than that of the formation of the acid just described, another change, resulting in the production of a resinous substance, and in conjunction with it, of strong acetic acid. This change took place with oil of cinnamon, of mint, and others; and so much acetic acid was formed in the vessels, that not only did the atmosphere redden litmus paper powerfully, but, by neutralizing the acid with potash, a portion of acetate of potash was obtained.

Upon subjecting the essential oils to a very low temperature, crystals formed in them, and ultimately a separation of them into two parts was effected, very analogous to the separation of fixed oils into stearine and elaine made by Chevreul. In imitation of that chemist, M. Bizio has given names to the two portions into which he has separated essential oils, calling the most fluid part *Igrusina*, and the solid part *Serusina*.

Igrusina is fluid at the lowest possible artificial temperature: it dissolves in alcohol, evaporates when heated, and leaves no residuum. When gradually changed by the action of oxygen, it becomes denser, and ultimately is changed into resin and acetic acid.

Serusina, obtained from the oil of roses and anise, is solid at 60° Fahr.; that from the oils of mint, orange, lemon, valerian, lavender, &c., require a temperature as low as 4° Fahr. for solidity. It crystallizes in prismatic needles, and sometimes in plates. It is soluble in alcohol, and those specimens that are solid at moderate temperatures may be obtained in crystals from the alcoholic solution. If *serusina* be exposed to the air, gradual action occurs, the substance becomes denser and acquires a higher point of fusion, and ultimately that peculiar substance appears to be formed which Brugnatelli found in oil of turpentine. When the serusina is obtained from those oils which produce the solid acid above described, it then undergoes a different change to that referred to, and gives rise to the production of that peculiar acid by exposure to oxygen or air.—*Giornale di Fisica*, ix. 360.

27. *Change of Oil of Turpentine by exposure to Air.*—Some oil of turpentine which had been exposed to air, was distilled by MM. Boissenot and Perset, who obtained a very acid liquid from it, which consisted of acetic acid, and a substance which crystallized

upon cooling. These crystals were colourless and transparent, inodorous, insipid, very slightly soluble in cold water, but very soluble in hot water; they were similarly affected by alcohol and ether. Strong acids dissolved this substance, but it was not affected by alkalies. It was formed of oxygen, hydrogen, and carbon, and does not exist in oil of turpentine recently distilled.—*Bull. Univ.* A. vii. 154.

28. *Preparation of Cafeine, by* M. Garot.—The coffee was broken and then digested in boiling water; the liquor obtained was brown, and being treated with acetate of lead, yielded an abundant greenish precipitate: this being removed by a filter, left a yellow liquor, through which a current of sulphuretted hydrogen was passed to separate a little lead remaining in it: the free acid left, was then saturated by ammonia, and the clarified liquor, when evaporated, gave crystals of cafeine.—*Jour de Phar.*

29. *Black Dye and Ink prepared with Logwood.*—The following is a process for the preparation of a black dye, for which a patent was taken out at Vienna, by Mr. Honig. Logwood is to be boiled several times in water, and a little subcarbonate of potash to be added to the decoctions, the quantity being so moderated that it shall not change the colour to blue; the stuff to be dyed is then to be plunged into this bath. This stuff may be either animal or vegetable. When it is well impregnated with colouring matter, it is to be withdrawn, and, without being exposed to air, is to be introduced into a solution of green vitriol, and left there until it has obtained the desired black hue. In preparing the ink, the decoction of logwood is used in place of the infusion of galls.

30. *Improvement in the Manufacture of Varnishes.*—It is usual in the manufacture of spirit varnishes, to mix glass or sand with the resin, for the purpose of affording ready access of the alcohol to all parts of the solid mass. M. Ferrari, however, recommends that in place of these substances, a coarsely-powdered charcoal should be used; for the glass or sand frequently tends to aggregate the resin at the bottom of the vessels, and protect it from the solvent, whilst, on the contrary, the charcoal rather tends to raise and divide it. The most advantageous proportion appears to be about one ounce of charcoal to one pound of the spirit or the oil of turpentine used.—*Giornale de Fisica*, ix. p. 36.

31. *Substances obtained from Castor Oil, by* MM. Bussy and Lecanu.—These chemists have obtained three new fatty acids from castor oil. One called *ricinic acid*, is fusible at 72° Fahr.; another termed *elaiodic acid*, is fluid below 32° Fahr.; and the third, or *Margaritic acid*, crystallizes in fine scales, and is not fusible below 264° Fahr. These acids are volatile, more or less soluble in alcohol, and

insoluble in water; they form peculiar salts with several bases, and especially with magnesia and oxide of lead.

When castor oil is distilled, a small quantity of gas, water, acetic acid, a colourless crystallizable volatile oil, ricinic, and elaiodic acids are obtained, and a solid substance remains in the retort. The quantities of acid and volatile oil are nearly equal, and constitute almost a third of the oil employed; the remaining two-thirds consist of the solid matter. This is a curious substance, is of a yellowish colour, cellular, and resembles the crumb of new bread. It is insoluble in water, alcohol, ether, the volatile and fixed oils. It is dissolved by alkalies, with which it forms a kind of soap. It requires a high temperature for its decomposition, inflames when exposed to a burning body, and burns readily without melting.

When castor oil is treated with solution of potash or soda, it saponifies more readily than olive oil, and ricinates, elaiodates, margaritates with glycerin are formed: no other products appear: the glycerin amounts to about one fifteenth part of the oil, the margaritic acid to about one thousandth. These salts are very soluble in water, and act like ordinary soaps.—*Phil. Mag.* N. S. i. 313.

32. *Distillation of fatty Bodies, by* MM. Bussy and Lecanu.—Spermaceti is a substance containing neither elaine nor stearine; yet, by the action of alkalies, portions of margaric and oleic acids are formed from it. Being distilled, it gave 97.5 per cent. of solid matter, and 2.5 parts of loss due principally to gas: 80 parts of the solid portion was colourless, nacreous, and fusible at 73° Fahr. Being examined by the action of water, alcohol, baryta, acids, &c., it gave spermaceti unchanged, colourless liquid oil, oleic, margaric and acetic acids, water, odorous matter, yellow matter, and an empyreumatic yellow oil. The volatilized portion of spermaceti being re-distilled, underwent similar changes, and in all cases margaric and oleic acids were produced.

Cholesterine being distilled, passed over into the receiver, leaving scarcely an atom of charcoal behind it; and the product being examined, was found to contain no proportion of the fatty acids, nothing being produced which could affect litmus paper, and all the properties of the cholsterine being preserved.

Ethal obtained, with great care, and free from barytic salts, when distilled, resembled cholesterine; no acids were formed, and the ethal passed over and condensed unchanged. These results induce the authors of this memoir to observe, that distillation may be resorted to as well as the action of alkalies, for the purpose of establishing distinctions between fatty and analogous bodies.—*Annales de Chimie*, xxxiv. 57.

33. *Action of Nitric Acid on Hogs' Lard.*—Two ounces of hogs' lard were mixed by MM. Bussy and Lecanu, with four times its weight of hot and concentrated nitric acid, and retained hot for one

hour, after which, the temperature was allowed to fall, and the fatty matter separated; it was of a yellow colour, without odour, and softer than the fatty matter employed. Being perfectly washed with distilled water, it was then put into alcohol, which dissolved nearly the whole of it: the solution reddened litmus paper, and being evaporated on a water-bath yielded a yellow residuum, which being placed in filtering paper, was submitted to the press. The pressure separated a yellow liquid, very acid, soluble in all proportions in alcohol and solution of potash, and susceptible of forming a compound with baryta, insoluble in water or alcohol. The solid matter left in the press being agitated with hot baryta water, formed an insoluble salt, which was then washed with hot alcohol, to separate any fatty matter not acid; and the salt thus purified was decomposed by weak muriatic acid; a solid fatty matter was obtained, which being washed with distilled water until the washings caused no change on nitrate of silver or vegetable colours, was then dissolved in alcohol, and crystallized.

In this state it was colourless, inodorous, and lighter than water. It fuzed at 62° C. (144° F.) Boiling alcohol dissolved it with facility, and deposited it on cooling in beautiful nacreous crystals. It reddened moistened litmus paper, and combined directly with potash and baryta, forming with the first a compound analogous to common soap and soluble like it in alcohol and water; and with the baryta a pulverulent salt insoluble in these fluids.

Hence it appears that the lard, when acted upon by nitric acid, is converted into a mixture of oleic and margaric acids; and, as analogy and analytical experiments permit the extension of the conclusion founded upon these results to all fatty bodies consisting of stearine and elaine, it may be stated that the property of converting these bodies into margaric and oleic acids, first observed in the alkalies, then in sulphuric acid, in oxygen, and in heat, also belongs to nitric acid. From this, we may be led to suppose that similar phenomena occur whenever the order of the elements in stearine and elaine are disturbed, in whatever manner that may be effected.—*Bull. Univ.* E. vii. 162.

34. *Ozmazome.*—M. Peretti dissolved ozmazome, obtained from beef, in alcohol, and purified it by animal charcoal; the alcoholic solution then supplied him with small transparent crystals, resembling those of oxalic acid in many properties, but differing in their action on proto-sulphate of iron and nitrate of silver. It precipitated the first of a grey colour, and the second of a bright yellow hue; whilst oxalic acid, and oxalates, precipitated the first yellow, and the second white.—*Giorn. Arcad.*

III. Natural History.

1. *On Death by Insufflation, or the Passing of Air into the Lungs.*—M. Leroy d'Etiolles some time since pointed out the danger of throwing air into the lungs of drowned persons, and stated that the operation, when performed simply by the mouth but strongly, was sufficient to occasion the death of certain animals, as dogs, cats, and rabbits; since then, M. Leroy has continued his experiments, and to render them more conclusive, has employed animals approximating more nearly in size to man. Seven sheep were killed in this manner; four of them died immediately, and the other three lived only a quarter of an hour. On inspecting the lungs of these animals, an unexpected result was observed; for it was found that the tearing of the pulmonary cells was not the cause of death, as had been supposed: none of the sheep who died instantly offered this appearance. In the three which died more slowly, there was certainly found a quantity of air in the thoracic cavity, but all circumstances lead to the conclusion that the accident was not the cause of their death, and that they died of asphyxia, like the others. The cause of this asphyxia has not as yet been discovered. Great difficulty occurs in examining the lesions which may have been occasioned in the lungs by the operation of throwing in air, because, according to M. Leroy d'Etiolles, nothing is more rare than to find a sheep whose lungs are unaffected by some disease, the effects of which may not be confounded with those produced by the operation. —*Bul. Univ.* C. x. 37.

2. *Remarks on the Human Voice, by* D. Liskovius *of Leipsig.*—The remarks by M. Liskovius are in the form of a reply to objections advanced by M. Rudolphi against previous statements and opinions of the former. M. Liskovius had said that the vocal cords lengthened as the aperture of the glottis dilated. M. Rudolphi remarked upon this, that during ordinary inspiration and expiration the glottis is so much open that no sound can be heard; and, consequently, that to produce even the lowest notes of the gamut a dilatation greater than this cannot be required, as it would have no other effect than to prevent the formation of sound altogether. In reply, M. Liskovius remarks, that when in a state of repose, the larynx holds a middle place between its greatest elevation and depression, and that the glottis also is in an intermediate state between its greatest contraction and extension; the more the larynx descends, the more the glottis is dilated, and the greater the degree to which the former ascends, so much the more is the latter contracted: if, therefore, the glottis were the most dilated in a state of repose, the larynx should, at the same time, be at the maximum of depression, and its depressing and dilating muscles would be at the maximum of contraction; in such a case, each person would talk

in the lowest tones of his voice; as the state necessary to produce them would be that which would offer itself.

That ordinary respiration produces no sound, does not depend upon the too dilated state of the glottis, but upon the feeble impulse given to the column of air traversing the larynx; a point neglected by M. Rudolphi. As to the falsette, it is produced by the anterior part of the glottis, the ligaments being at such time tense and drawn together, whilst the open or pectoral voice is produced by the approximation of the open cords, without any simultaneous tension of them. M. Liskovius contends also, that the falsette is a well-characterized species of voice, and not an imperfect voice, as has been supposed by Haller, Kempelin, and Rudolphi. The conclusions drawn, and principles established, are founded upon anatomical examinations of, and experiments upon, the human body.—*Archiv. für Anat.* 1826, p. 116.

3. *Sabulous formation in the Brain.*—Dr. Bergmann, of Celle, has sent a memoir to the Royal Society of Gottingen, in which he describes twenty cases of earthy granulations occurring in the plexus choroïdes of the lateral ventricals of the brains of insane persons. The various lesions observed by the author in the brains of these patients, induces him to think that this plexus belongs to the principal organs of the animal. The earthy granulations which were found in and upon the surface of the plexus, resemble those of the pineal gland. M. Stromeyer analysed both varieties, and found their constitution similar, and of the following nature:—phosphate of lime in large quantity, phosphate of magnesia in small proportion, traces of carbonate of lime, and an animal substance of an albuminous nature.—*Bull. Univ.* C. x. p. 128.

4. *Analysis of a Ferruginous Renal Calculus, by* M. Boussingault.—The calculus was sent from Bogota by Dr. Roulin, to M. Boussingault; it weighed rather more than a gramme (15.4 grains), was about the size of a nut, and considerably resembled certain mineral states of iron sand: being analysed, it yielded the following constitutents:—

Peroxide of iron	38.81
Alumina	23.00
Silica	17.25
Lime	8.02
Water	10.89
Loss	2.03
	100.00

Several other calculi of similar composition and appearance were afterwards voided by the same person.—*Bull. Univ.* c. x. p. 128.

5. *Production and Preparation of Pearls.*—For Mr. Gray's observations on the artificial production of pearls, see p. 167, vol. xix., and p. 412, vol. xx., of the first series of this Journal. M. Geiger has made some experiments upon these substances: one object was to ascertain the probability that the Persians caused birds to swallow the pearls for the purpose of removing the crust with which their surface is sometimes covered. From the trials made with two cocks, M. Geiger thinks that the practice may be rendered useful: he thinks also that the formation of pearls is always due to the introduction of extraneous bodies into the shells of the fish; he believes that they may be made artificially, with advantage, and is told that fine pearls are made in Finland in this manner.—*Bull. Univ.* C. x. p. 178.

6. *Peculiar Cases of the Use of Milk as Food.*—A giraffe, which was sent to the King of France by the Pacha of Egypt, was observed never to drink the smallest quantity of water, but only milk. This odd circumstance is explained by the person who describes its habits and manners, as resulting from the circumstance that being taken whilst young, it was, probably, supplied with milk, which not having been discontinued, has occasioned this permanent inclination in the animal. It appears very probable, that animals which drink but little naturally, will not drink water, if a sufficient quantity of milk be supplied to them. Milk was offered to two young asses which had been separated from their dam for some time, and they drank it with pleasure. It was then offered to a young mule, and to a horse five years of age; both drank of it. Being offered to a monkey, it seemed never to have taken enough. Pigs, dogs, cats, and rats, drink milk with avidity. "I will quote on this occasion a curious fact, but little known,—that of a goat which sucked itself, and which was, with difficulty, broken of this bad habit." Now, as there are so many animals which like milk, without having preserved the habit of drinking it, it will not seem surprising that the giraffe, a herbivorous animal, which has been continually supplied with this drink, should prefer it to all others.—*Mem. du Museum*, xiv. p. 74.

7. *Preparations of Henbane and Belladonna, by* M. Buchner.—M. Buchner prefers the well-prepared extracts of these plants in practice, to the purified and peculiar principles in their crystalline forms. His process for preparing the extracts, consists in pulverising the plant, and digesting it in alcohol of a specific gravity of .847 for several hours; the liquid is then to be filtered, distilled until two-thirds have passed over, and the residue evaporated until of the consistence of extract. It is then to be re-dissolved in water, filtered and re-evaporated to dryness. The extracts thus obtained are of a yellowish colour, transparent in thin plates, possessing the odour of the plant, and soluble in water and alcohol.

Dr. Reisinger, who has employed these preparations, concludes

that it is advantageous to employ the dry plants in the preparation of the extracts. 2. That the roots of henbane give a less active extract than the leaves. 3. That the seeds of this plant furnish the most active extract, and contain much malate of hyoscyama. 4. That the seeds of belladonna yield a more active narcotic extract than the leaves. 5. That the extract obtained from the roots of the belladonna is preferable to that from the leaves, but not so good as that from the seeds.—*Repert. fur die Pharmacie.*

8. *On the Culture of Bees in Forests, by* M. Buttner.—It has been a custom in Livonia, from time immemorial, to make cavities in the trees of a forest for the purpose of receiving and rearing the swarms of bees. Some of the proprietors have hundreds and even thousands of bee trees. Those which are chosen for this use are large oaks, firs, pines, alders, &c. It has been objected to this system, that it destroys the forests and diminishes the quantity of building wood, but M. Buttner observes that it is not necessary to choose the finest trunks, and that stunted trees are equally serviceable for this use, if they have sufficient size. He states also that a bee tree is worth more than if sold for wood; that the old hollow trees which will serve for an age or two, spread seed around, and cause the production of young suckers, which would be obtained with difficulty, by destroying the old trunks. He adds, that the pure air of the higher regions agrees better with the bees than the air inclosed in hives which receive the exhalations of the earth, and in which contagious diseases sometimes make great ravages. The proof he offers is, that when garden bees swarm they are directed instinctively towards the woods, whilst the bees of the wood never swarm towards the gardens.—*Bull. Univ.* D. vii. 34.

9. *Signs of Increase, Maturity, and Decay in Trees, by* M. Baudrillart.—The qualities of wood depend much on the state of the tree when cut down. It appears from the experiments of M. Hartig upon wood applied as fuel, that trees which have attained maturity without passing into decay, are the best for the production of heat. Thus the value of an elm of one hundred years is to that of one of thirty years, as twelve is to nine; that of an ash of one hundred years to one of thirty years, as fifteen to eleven. When the trees begin to decay, their value rapidly diminishes; thus, if an oak of two hundred years yields wood worth fifteen francs per corde, a tree of the same kind passing to decay yields wood only worth twelve francs. When the wood is used for other purposes, the advantages conferred by a mature but healthy state are still more considerable.

The common elm, growing in a forest and in good earth, acquires its full increase in about one hundred and fifty years, but it will live many ages, even five or six hundred years. Large forest elms are cut down with advantage when of an age between one hundred

and one hundred and thirty years, and then furnish a large quantity of building wood. The duration of the life of the elm depends much upon the soil; in a dry soil it becomes aged, as it were, in forty, fifty, or sixty years. Elms which have been lopped live for a shorter period than the others. Those which grow by the road side, or in thin plantations, may be cut when seventy or eighty years of age. In general, the increase of hard woods, as the oak and the elm, is small at first; it successively augments until the twentieth or twenty-fifth year, is then uniform until the age of sixty to eighty years, after which it sensibly diminishes.

For these and other reasons, it is important that trees should be cut down when they are at their mature state, and not simply when they undergo no further increase. When the period has arrived after which the increase of the tree would be less and less from year to year, then the tree should be felled, for no advantage accrues from its remaining longer in the ground. The indications of the mature state of a tree are by no means so evident as those of decay, but still certain signs of these states, as well as of the vigorous condition of the tree, may always be observed.

I. *Signs announcing the vigour of a tree.*—The branches, especially towards the top, are vigorous: the annual shoots strong and long; the leaves green, vigorous and thick, principally at the summit, and falling late in autumn; the bark is clear, fine, united, and nearly of the same colour from the foot to the large branches. If at the bottom of the veins or divisions of the thick bark there appear smaller divisions which follow from below upwards in the direction of the fibres, and live bark be observed at the bottom of these divisions, it is an indication that the tree is very vigorous and rapidly increasing in size. If some of the lower branches stifled by the others are yellow, languishing, and even dead, this is an accidental effect, and is no proof of the languor of the tree. Finally, it is a sign of vigour when branches are seen at the summit of the tree rising above and being much longer than the others; but it is to be observed that all trees with round heads do not throw out branches with equal force.

II. *Signs which indicate that the tree is mature.*—Generally the head of the tree is rounded; the shoots diminish in length each year, and the furthest shoots add to the length of the branches only by the length of the bud; the leaves are put forth early in spring and become yellow in autumn before those of vigorous trees, and at this time the lower leaves are greener than the upper. The branches incline towards the horizon, and form angles sometimes of sixty or seventy degrees. These apparent signs, and the thinness of the layer deposited by the sap, indicate that the tree makes but small additions to itself, and now it should be cut down. The nature of the earth should be examined, as well also as the kind of tree, to enable a judgment whether the tree should be left to in-

crease still further, or whether it will be more proper to fell it. An exact age cannot be assigned for each species; but it has been observed, that an elm situated in an insulated plantation may be felled with advantage when between seventy and eighty years of age.

III. *Signs of decay in a tree.* When a tree becomes crowned, i.e. when the upper branches die, it infallibly indicates, especially for isolated trees, that the central wood is undergoing alteration, and the tree passing to decay. When the bark separates from the wood, or when it is divided by separations which pass across it, the tree is in a considerable state of degradation. When the bark is loaded with moss, lichen, or fungi, or is marked with black or red spots, these signs of alteration in the bark justify suspicions of alterations in the wood within. When sap is seen to flow from clefts in the bark, it is a sign that the trees will soon die. As to wounds or gutterings, these defects may arise from local causes, and are not necessarily the result of old age.—*Biblioth. Phys. Econom.* 1826, p. 13.

10. *Structure of Plants.*—M. Raspael has endeavoured to ascertain what alterations are occasioned in a lengthened time by water, acids, and heat, upon feculous teguments, and, consequently, upon all vegetable tissues, which, according to him, are merely vesicles similar to the teguments. The following are some of the results which M. Raspael says he has obtained. After an ebullition continued for eighteen hours, the teguments began to appear as a layer of granules: continuing the ebullition, these granules were successively detached from each other until the liquid no longer offered traces of teguments, but merely globules from the $\frac{1}{300}$th to $\frac{1}{400}$th of a millemeter in diameter. These resisted ebullition during eight hours per day, for a whole month. The experiment was made in vessels almost close.

The liquid being put into a flask half filled with air, was left; the granules required fifteen days for their precipitation, whilst the untouched feculous integuments were almost entirely precipitated on the third day, according to the quantity of fecula submitted to ebullition. The liquid part of the contents of the flask did not cease to colour iodine till a month after; the precipitated part continued to become coloured purple by iodine. Water without elevation of temperature is competent to disintegrate the globules composing feculous teguments, but requires two or three months to manifest the action.

Muriatic acid, out of contact of air, produces the same effect, but only after a month's action, provided the experiment has not been exposed to heat or light, the only difference in the experiment is, that the globules become black and charred, but still remain vesicular.

Thus M. Raspael's idea relative to the formation of vegetable cellular tissue, is supposed to be proved, the evidence being such that any one can refer to it at pleasure. The cellular tissue is composed of teguments, these of agglutinated globules, and these may be conceived to be composed of other globules, and so on to infinity.

The same phenomena being sought for in germinating grain were perfectly observed, and another analogy between the grains of fecula and many other organic grains established. M. Raspael found that the grains of fecula gradually emptied by the progress of germination were organised precisely like grains of pollen, and that pollenic granulations might be observed in their centre with perfect facility. This fact led him to study the grains of pollen particularly, and being occupied at the same time with *lupuline*, numerous analogies were elicited, of which the following are the principal.

The grains of *pollen* are composed of a vesicle, containing a variable number of other vesicles which enclose the granules that, when detached from their surface, contribute to fecundation. *Lupuline* is spread over all the yellow leaves of the hop, as well as over the scaly cones of the female flower; and they are detached only as these leaves are developed. Guettard designated these grains of lupuline by the name of *vesicular glands*, which are found on a multitude of other vegetables.

The glands of other vegetables have the same organization, though they do not all contain the same substances. Cortical pores are merely cells, which, according to an organization identical with that of pollen and glands, contain one or two vesicles filled with granulations.

Generally, leaves which have not vesicular glands are supplied with these pretended cortical pores. Those which have no cortical pores are supplied with glands, more or less modified in their structure.

Every thing induces M. Raspael to believe that the lupuline, the glands, and the cortical pores, are destined, like the grains of pollen, to cause the fecundation of the buds; and that, in experiments on generation by means of the two sexes, it is these organs which have sometimes replaced the action of the stamens, and puzzled the sagacity of observers.—*Bull. Philom.* 1826, p. 155.

11. *Experiments on the Fecundation of Vegetables, by* M. Gaertner.—In consequence of the doubts and discussions which have arisen in Germany relative to the sexual system in plants, M. Gaertner has been induced to make a multitude of experiments, for the purpose of obtaining undoubted knowledge upon this point. The experiments have been made upon four different families of plants, containing thirty species, belonging to sixteen genera: although about six hundred in number, they are still to be con-

tinued. They relate—1. To the fecundation of the plant naturally or artificially, by its proper pollen: 2. To fecundation by the pollen of another plant: 3. To the action of pulverulent substances; as sublimed sulphur, pulverised charcoal, carbonate of magnesia, lycopodium, &c.: and 4. To the duration and mode of action of the pollen of one plant upon the ovaria of another; principally to ascertain whether any influence would be exerted upon the form, colour, or time of maturity of fruits and seeds.

The following are a few of the facts ascertained. A microscopic quantity of the pollen proper to a plant being placed upon its pistil, has so great an influence as entirely to destroy the action of a large mass of the pollen of another plant, even though of a very near species: the proper pollen of a plant being applied by means of a pencil to the stigma, becomes so exactly attached to it, that it is difficult to remove it without injuring the stigma: with the pollen of another plant it is quite otherwise; the stigma then appropriates it with difficulty and slowly, and the difficulty increases with the difference between the species. When the fecundation of one plant is occasioned by the pollen of another, the pollen applied to the stigma disappears in a time which, all other circumstances remaining the same, varies as the affinity between the species is more or less. When once the pistil is saturated with fecundating matter, that which is afterwards applied undergoes no change either of form or colour. In natural fecundation, the stigma loses its fullness and freshness as soon as it is saturated with fecundating matter; in the cross fecundation, the stigma remains unchanged for a much longer time, and even sometimes seems to be renewed in vigour. In the latter case, the pollen which is reapplied disappears until the stigma loses its freshness. The foreign pollen retards rather than accelerates fecundation.

M. Gaertner has extended his observations much further, even to observing the changes in the corolla; on the secretion of the juices of the plant; on the period of ripening; on the different states of the seeds, &c. &c.—*Bul. Univ.* B. x. 77.

12. *On a natural Botanical Arrangement dependant on Chemical Characters.*—Many naturalists have endeavoured to establish an analogy between the external forms of plants and their medicinal characters. Dr. Runge has undertaken to prove by analysis, that there is a chemical relation—1. Between the different parts of a plant (species); 2. Between the different species of a genus; 3. Between the different genera of a family. To the first point, he quotes the experiments of Vauquelin on the Hippocastan, and his own upon Belladonna and the oak; from which it results, that tannin is found in every part of the first and third, even to the cotyledons of the latter, and the narcotic principle in every part of the second. Relative to the second point, he observes that prussic acid has been found in the kernels of all the species belonging to

the genus *prunus*, and a yellow colouring matter in the bark of the roots of the species belonging to the genus *galium*. On the third point, he cites the strychnia and veratria, found by MM. Pelletier and Caventou; the first in most of the strychnos, and the second in many colchicums. The author thinks that the same principle may be applied to the different families composing any group or order.

In his experimental results, M. Runge has found that all the species of semiflosculuses, flosculuses, *radiola*, *diphyscium*, *valeriana*, and *scabiosa*, gave him one common principle, but contained in different parts, as the root, stem, leaves, &c. This substance resembles the vegetable acids which form soluble and uncrystallizable combinations with alkalies, and insoluble precipitates with metallic oxides. In the first state it has a yellow colour, but by the addition of oxygen becomes of a bluish green colour. Being combined in these two states with oxide of lead, the following results were obtained:

Colourless acid	52.9	Coloured acid	54.6
Oxide of lead	47.1	Oxide of lead	45.4
	100.0		100.0

Having found this principle in neighbouring families, it was afterwards sought for in those which were more distant, and was found in the umbelliferous plants, the caprifolias and the plantains; but he found no trace of it in the genera *campanula* and *rubia*. On the other hand, in these he discovered a principle which he found in no other family.—*Isis*, 1826.

13. *On the Application of a Ligature to Fruit Trees, and to Vines, by* Dr. J. W. Fischer.—The process recommended by the author, is intended to be applied in place of the annular incision, which is made round fruit-trees, for the purpose of increasing the growth and the quantity of fruit. In the same place where the annular incision would otherwise be made, an iron wire is to be passed tightly round the tree two or three times, and the ends twisted together. This operation is to be performed in winter, especially in February, and before the sap is in circulation. It is unnecessary to observe, that the ligature should be applied to those branches or young trees which are to be rendered more productive. In the summer following, after the flowering, and when the fruit begins to increase in size, the ligature is to be removed, so that the wound which may have been occasioned in the bark may cicatrize. These ligatures may be repeated every year, their places being changed: they are said to produce all the good effects of ringing, and to be free from the objections which may be urged against that process. In place of iron wire, a hemp-string well soaked in oil may be used.—*Bull. Univ.* D. vii. 109.

14. *Method of reviving Plants, Shoots, Sprigs, Slips, and other*

Vegetables.—This is called a proved method of reviving plants, &c., when their leaves and buds are faded, and their bark and roots hard and nearly dry, by M. de Droste of Hülshof. The directions are to dissolve camphor to saturation in alcohol, adding the former until it remains solid at the bottom of the latter; a sufficient quantity of rain or river water is then to have the alcoholic solution added to it, in the proportion of four drops to one ounce of water. As the camphor comes in contact with the water, it will form a thin solid film, which is to be well beaten up with the water: for a short time the camphor will float in the water in small flocculi, but will ultimately combine with the fluid and disappear.

Plants which have been removed from the earth, and have suffered by a journey or otherwise, should be plunged into this camphorated water, so that they may be entirely covered: in about two, or at most three hours, the contracted leaves will expand again; the young faded and dependant shoots will erect themselves, and the dried bark will become smooth and full. That being effected, the plant is to be placed in good earth, copiously watered with rain or river water, and protected from the too powerful action of the sun, until the roots have taken good hold of the ground.

When large plants, as trees, are to be revived, their roots are to be plunged into the camphorated water for three hours; the trunk and even the head of the tree being frequently wetted with the same water, so as to retain them in a properly moistened state. But it is always best, if possible, to immerse the whole of the plant. Shoots, sprigs, slips, and roots, are to be treated in a similar manner.

If plants thus treated are not restored in four hours, their death may be considered as certain, for they cannot be recalled to life by any artificial means. They should, consequently, never be left more than four hours in the camphorated bath; because the exciting action of the camphor, when it is continued for a longer period, may injure the plants, instead of doing good to them. It is not necessary to say that the final prosperity of the plants, thus re-animated by the camphorated water, must depend upon the particular properties of the former, the state of their roots, and the pains that are taken with them. The camphor produces no other effect than to restore life to plants nearly dead: after that, all proceeds according to the ordinary laws, and their ultimate state must be left to art and nature.—*Verhandl des Vereins, &c.*

15. *Antient Glass Bottles.*—Amongst the curious and interesting objects lately discovered in the excavations at Pompeii, are five glass bottles, in some of which were olives, in an extraordinary state of preservation. These olives were soft and pasty, but entire, and had the same form with those called Spanish olives; they had a strong rancid odour, and a bitter taste, leaving a biting astringent sensation upon the tongue. A part of these olives have been analysed, and the rest have been deposited at the museum, in the same bottles in which they were found.—*Bul. Univ.* G. vii. 142.

16. *Natural Prognostics of the Weather, by* M. Waldeck.——*Signs of Fine Weather:*—1st, *By Birds.* When the *alcido hispida* and ducks leave the earth and fly towards the sea; when the kites and bitterns cry as they fly; the swallows fly at great heights, because the flies then keep in the upper regions; the *apodes* fly after each other in the air till late in the evening, with vivacity of noise; the ravens and sparrow-hawks cry frequently and loudly; the turtle-doves coo softly; the robin rises in the air singing; the owl hoots; the wren (sylvia troglodytes) sings in the morning until nine or ten o'clock, and after mid-day till four or five o'clock —beyond this time their song announces rain. 2ndly, *By other Animals.* Frogs contained in glasses climb up their sides; the glow-worms fly about in great numbers in the evening; insects and flies play in the air after sun-set; the bat appears late; the spiders spin tranquilly, and extend their nets to a distance.

Indications of Rain.—1st, *By Birds.* When the large black sea-mew, the cormorants, aquatic birds, and birds generally, go to the rivers, and water and bathe noisily; ducks, geese, and moor-hens plunge into the water, flapping it about with much noise; wild geese fly high in the air and in disorder; the plovers become restless, flying here and there, and uttering their peculiar cry; the ravens and the rooks assemble in groups, and then almost immediately separate; the ravens in the morning, and the rooks in the evening, utter continual cries, and walk solitarily on the ground; the swallows fly low in the air; the magpies cry much at other seasons than at pairing time; domestic birds rub themselves in the dust; partridges, pigeons, and smaller birds, bathe themselves in the sand; the cock crows immediately after sun-set (when, on the contrary, the cock walks about during rain, it is a sign it will soon cease); the melancholy cry of the chaffinch is heard; the wood-lark, linnet, sparrow, and robin, cry or sing during the morning; the peacocks and owls cry more frequently and strongly than usual during the night; the poultry are longer engaged in searching out the insects beneath their feathers, the latter penetrating more deeply into the skin. 2ndly, *By other Animals.* When the cattle pant for air towards mid-day; pastured cattle, sheep, and goats, leap much and quarrel with each other; pigs are restless and disperse their food; cats rub their ears and press their bodies against obstacles; dogs become restless, scratch the ground, eat grass, and bark in a growling manner; the foxes bark; the wolves howl; the moles raise the earth higher than ordinary; the frogs croak much, and hide themselves in the meadows; the bats do not leave their retreats in the evening; the spiders work but little, spin short threads, and retire to their corners; the flies bite horses and cattle on the legs, are agitated, and fly confusedly together; the fish (*cobitis fossilis*) trouble the waters; and the worms disturb the earth.

It is considered as a presage of *wind*, when aquatic birds of the sea and marshes fly together towards the land and play, especially

in the morning; birds at sea take shelter on vessels; wild geese fly very high, and in bands, going towards the east; water-fowls cry, and are agitated; the lapwing cries loudly; the king-fisher flies towards the earth: the rooks (corvus frugilegus) pass rapidly through the air, and play on the borders of water. It is well known that hares have a presentiment of wind, and will often set ten hours in advance on the places where it will blow.—*Sylvan, Jahrbuch für Fast Maenner.*

17. *On ancient Aërolites, by* Dr. Noggerath.—The following notice, which has been partly omitted by Chladni, may be found in the *Laboratorium Chimicum* of Becher, Frankfort, 1680. Albertus Magnus relates from Avicenna, that a piece of iron of the weight of one hundred pounds fell from heaven; it is that which Chladni places in the year 1009, on the borders of the Caspian Sea. Another piece fell at Grimma, in the country of Meissen, which is placed by Chladni between 1540 and 1550. Petermann Eterlein writes in his *Chronica Helvetica* that there fell from heaven during a storm, a mass of iron sixteen feet in length, fifteen in width, and two in thickness: he computes that this mass should weigh 48,000 pounds. Paul Merula says in his *Cosmographie*, that six iron hatches fell from heaven: he adds that these were probably stones in the form of hatches, called by the Germans *donneraxt*. Chladni appears to have had no knowledge of the last two facts.—*Journal für Chimie.*

18. *Fresh Water found at Sea.*—Mr. Buchanan relates that "In the beginning of September 1824, I embarked with the other officers of our regiment, in a country ship, for Chittagong. We sailed out of the Madras roads with a fair wind, which continued for four days, but on the fifth we were becalmed, and continued so for fourteen days, having had only once or twice a very slight breeze, which never lasted longer than a few hours. It was towards the end of this calm that I observed a very strange appearance on the surface of the glassy ocean. It seemed to be furrowed in several directions, and much agitated in these furrows; so that when the ship was drifted into these parts, she was driven about in all directions. On the night of the 14th, a breeze sprang up. Owing to our unexpectedly tedious passage, we ran short of provisions, particularly water. You may suppose what was our joy and astonishment the next morning, in taking up the water alongside to wash decks, to find that it was fresh and much more palatable than that which remained in our casks, which were immediately replenished with it. By this day's observation, we were one hundred and twenty miles from Chittagong, and about one hundred miles from the nearest part of the Juderbunds. The water was of a more yellow tinge than in most parts of the bay, and those who drank a great deal of it suffered from it afterwards.—*Jameson's Jour.* 1827, p. 369.

METEOROLOGICAL DIARY for the Months of March, April, and May, 1827, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For MARCH, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Thursday ..	1	47	51	29.06	29.22	WSW	W
Friday	2	34	39	29.22	29.29	W	W
Saturday...	3	33	47	29.29	29.12	SW	SW
Sunday	4	38	46	28.68	28.85	S	NW
Monday....	5	29	45	29.47	29.32	W	SW
Tuesday ...	6	38	48	28.92	28.70	SW	SW
Wednesday.	7	33	47	29.30	29.11	WbS	SE
Thursday ..	8	37	46	28.67	28.80	SSW	SW
Friday	9	28	40	29.20	29.80	W	NE
Saturday...	10	21	43	29.50	29.65	NE	W
Sunday	11	35	54	29.30	29.28	WbS	SW
Monday	12	42	51	29.34	29.67	WSW	W
Tuesday ...	13	42	52	29.58	29.68	W	WSW
Wednesday.	14	41	48	29.47	29.75	W	W
Thursday ..	15	39	45	29.49	29.53	W	NW
Friday	16	36.5	46	29.90	29.90	WNW	W
Saturday...	17	41	46	29.22	29.48	W	NbW
Sunday	18	32	43	29.90	30.00	NbW	N
Monday....	19	30	45	30.10	30.10	SE	SE
Tuesday ...	20	42	53	30.04	30.02	W	W
Wednesday.	21	44	51	29.90	29.89	W	W
Thursday ..	22	45	54	29.87	29.87	W	W
Friday	23	45	57	29.87	29.87	W	W
Saturday...	24	43	56	29.87	29.84	W	W
Sunday	25	43	47	29.80	29.87	W	W
Monday	26	29	47	30.06	30.00	NW	W
Tuesday ...	27	35	49	29.84	29.57	SW	SW
Wednesday.	28	38	52	29.50	29.07	WSW	SW
Thursday ..	29	36	48	28.94	29.13	W	W
Friday	30	34	47	29.20	29.50	W	NW
Saturday...	31	33	48	29.86	29.99	NW	NW

For APRIL, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Sunday	1	37	51	29.99	29.94	NW	SW
Monday....	2	45	68	29.94	29.92	W	W
Tuesday ...	3	43	55	29.92	29.83	W	W
Wednesday.	4	45	57	29.83	29.90	SW	SW
Thursday ..	5	41	59	29.90	29.87	SW	SE
Friday	6	47	63	29.78	29.80	SE	SbW
Saturday...	7	44	58	29.90	29.90	W	WSW
Sunday	8	34	58	30.17	30.13	W	SE
Monday....	9	41	59	29.99	29.80	E	SW
Tuesday ...	10	45	56	29.72	29.72	W	NE
Wednesday.	11	42	54	29.77	29.74	NE	E
Thursday ..	12	41	54	29.67	29.68	SE	WbN
Friday.....	13	34	56	29.87	30.00	WbN	NW
Saturday...	14	33	56	30.06	30.00	W	NW
Sunday	15	42	54	29.94	29.93	NW	NW
Monday....	16	40	52	29.93	29.94	NNE	NE
Tuesday ...	17	35	50	29.94	29.90	NE	ENE
Wednesday.	18	39	44	29.86	29.76	N	N
Thursday ..	19	41	51	29.70	29.59	NE	N
Friday	20	35	53	29.54	29.50	N	E
Saturday...	21	40.5	46	29.47	29.49	E	NE
Sunday	22	38	43	29.60	29.67	NE	NE
Monday	23	34.5	46	29.67	29.53	N	SE
Tuesday ...	24	33	47	29.40	29.40	SE	W
Wednesday.	25	30	48	29.47	29.77	W	WbS
Thursday ..	26	23	48	29.90	30.10	W	SW
Friday	27	28	55	30.10	30.10	W	ESE
Saturday...	28	32.5	62	30.02	29.90	ESE	ESE
Sunday	29	38	68	29.90	29.90	E	ESE
Monday	30	43	70.5	29.90	29.90	SE	W

For MAY, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Tuesday ...	1	47	66	29.90	29.90	WNW	NE
Wednesday.	2	46	61	29.90	29.82	NE	SE
Thursday ..	3	44	67	29.80	29.78	W	W
Friday	4	43	63	29.73	29.64	W	SW
Saturday...	5	51	63	29.50	29.40	SW	SW
Sunday	6	48	52	29.25	29.25	NW	NE
Monday....	7	39	52	29.48	29.62	NE	NE
Tuesday ...	8	30	49	29.70	29.73	NE	NE
Wednesday.	9	38	54	29.73	29.68	NE	SE
Thursday ..	10	39	58	29.68	29.64	SE	NE
Friday.....	11	34	56	29.64	29.78	NW	NE
Saturday...	12	36	54	29.90	29.93	NE	E
Sunday	13	33	54	29.89	29.78	E	NE
Monday....	14	38	52	29.78	29.73	NNE	N
Tuesday ...	15	43	60	29.65	29.61	NW	E
Wednesday.	16	43	63	29.50	29.30	E	ENE
Thursday ..	17	47	62	29.30	29.45	S	EbS
Friday.....	18	50	63	29.45	29.60	SE	SW
Saturday...	19	46	63	29.70	29.78	W	W
Sunday	20	37	70	29.90	29.90	SW	E
Monday....	21	42	63	29.93	29.98	E	W
Tuesday ...	22	49	66	29.98	29.87	SW	NW
Wednesday.	23	46	68.5	29.85	29.57	W	SW
Thursday ..	24	47	62	29.33	29.27	W	SW
Friday	25	38	60	29.24	29.24	SW	SW
Saturday...	26	41	63	29.26	29.30	SW	SW
Sunday	27	41	63.5	29.37	29.40	WSW	WbS
Monday....	28	49	64.5	29.47	29.54	WbS	SW
Tuesday ...	29	54	64	29.54	29.54	S	S
Wednesday.	30	50	66	29.67	29.67	SW	SW
Thursday ..	31	54	64	29.50	29.50	SbW	SW

INDEX.

Abraham, (Mr.) his mode of correcting the magnetism of watch-works, 223
Acids, new, obtained from animal substances by alkalies, 239
Acid, hydrocyanic, on the detection of, in animal bodies, 480
Achromatic microscopes, with a description of certain objects for trying their defining and penetrating power, 410
Action of nitric acid on hogs'-lard, 489
Aërolites, ancient notice of, 502
Agriculture, on the introduction of salt as an agent in, 337
Ainger, (Mr. A.) on the principle of security in the various kinds of locks, 212—remarks on suspension-bridges, 456
Air, heat evolved from, by compression, 228
Alcock, (Mr.) his account of the extensive application of the chlorurets of lime and soda as disinfecting agents, 211
Alcohol, method of destroying the empyreumatic odour of, 239
Ammonia, presence of, in the material oxide of iron, 481—muriate of, volcanic, 481
Ammonia and mercury, nitrate of, 482
Animal substances, new acids obtained from, by the action of alkalies, 239
Ancient glass-bottles, notice of, 500
Arago, (M.) remarks by, on the change of inclination in the dipping-needle, 124—on the electro-conducting power of metals, 470
Architecture of the metropolis, remarks on, 352
Arcs perpendicular to the meridian which are now measuring on the continent of Europe, account of, 177
Arrangement, natural botanical dependant on chemical characters, 498
Arsenic, preparation of the chloride of, 234
Asbestus, crystallization of, 241
Assamese poison, notice of, 211
Astronomical and nautical collections, 122, 434
Atlas Ethnographique du Globe, analysis of, 393
Atmospheric phenomena, 242
Atwater, (Mr.) on atmospheric phenomena at Ohio, U.S., 242
Aurora Borealis, its influence on the magnetic-needle, 229

Balls, velocity of, 468
Baron, (Dr. John) his life of Dr. Edward Jenner, with illustrations of his doctrines, and selections from his correspondence, 141
Bartholomew, (Mr.) his ornamental revolving-lamp, 210
Baths, warm, hints respecting the construction of, 62
Baumgartner, (Mr. A.) his experiments on the effect of the direct white light of the sun in making steel magnetic, 125
Bees, on the culture of in forests, 494
Bergmann, (Dr.) his remarks on a sabulous formation in the brain, 492
Biting-in, menstruum for, on plates of soft steel, 226
Blackadder, (Mr.) notice of his night-lamp, 210
Bones, on the best method of extracting the nutritious portion of, and its economical application, 56, 386
Boron, preparation of the chloride of, 234
Bottles of ancient glass, notice of, 500
Bouilland, (M.) his treatment of poisoned wounds, 245
Brain, notice of a sabulous formation in, 492
Brande, (W. T., Esq.) on the manufacture of dies for medals and coinage, 116
Brande's (Prof.) results of his observations on falling stars, 222
Bridges of suspension, remarks on the principles and construction of, 456
Brockedon (Mr.) observations by, on the tenacity of metals, 462
Bromine, on the existence and nature of its compounds, 233, 476
Burchell, (Mr.) notice of his travels in South America, 242

Cafeine, preparation of, 488
Calculus, analysis of, 492
Carbon and hydrogen, on the probable decomposition of certain gaseous compounds of, during sudden expansion, 204
Carbonic acid gas, deleterious effects of, 238
Castor-oil, substances obtained from, 488
Cathery, (Mr.) his method of etching on ivory, 226
Change of inclination of the dipping-needle, remarks on, 124
Charcoal, quantity of, from wood, 479
Chemical compounds produced by physical powers, 230
Chemistry, Ottley's Dictionary of, remarks on, 54
Chevreul and Gay-Lussac, (MM.) on new acids obtained from animal substances by alkalies, 239
Chile, orchideous plants of, remarks upon, 43
Chlorides or Chlorurets of lime and soda, account of the recent applications of, as disinfecting agents, 211
Chloride of arsenic, notice of its preparation, 234—of boron, notice of its preparation, 234—of Soda, preparation of, for medical use, 236
Chlorides, disinfecting, their mode of action, 237
Chlorine and compounds, chemical action of, 461

Chrome, oxide of, its preparation as a pigment, 235
Clement-Desormes, (M.) on vapour, 472
Cobwebs, (Artificial) for micrometers, notice of, 81
Coinage, on the manufacture of dies for, 116
Cold, effects of, 242
Collard, (M.) on the deleterious effects of carbonic acid gas, 238
Construction of warm-baths, hints respecting the, 62
Copper, (white) composition of, 483
Cultivation and improvement of edible fruits, remarks on, 265

Daniell, (Mr. F.) on the gas obtained by the decomposition of resin, 217
Decomposition of certain gaseous compounds of carbon and hydrogen during sudden expansion, 204
Defranc, (M.) his remarks on dense fogs, 241
Dense fogs, notice of, 241
Density of vapours and gases, table of, 233
Despretz, (M.) his claim to the discovery of chloride of boron, 243
Diamond microscope, 211
Diamond, uses of, in the arts, 464
Dichroite, new optical property of, 468
Dies for medals and coinage, remarks on the manufacture of, 116
Dipping needle, remarks on the change of inclination of, 124
Disinfectants, chemical remarks on, 371
Disinfecting chlorides, their modes of action, 237
Dumas, (M.) on earthy depositions from water influenced by the electricity of metals in contact, 230
——————— his table of the density of vapours and gases, 233—on chloride of boron, 234

Elastic mould for castings, notice of, 225
Electric discharges, transference of ponderable matter in, 472
Electro-chemical appearances, remarks on, 229
Elementary principles of the structure of language, illustrations of, 163
Europe, an account of the arcs perpendicular to the meridian, which are now measuring on the continent of, 177
Exercises, on the effect of, on the form, and their influence in preventing or curing distortions of the spine, 319

Falling stars, results of observations on, 222
Faraday's (M.) general account of the magnetic phenomena developed by metals in motion, 209—his account of the chemical action of chlorine and its compounds, 461—on the progress and present state of the Thames tunnel, 466
Fatty bodies, distillation of, 489
Fay, (Mr.) remarks on his mode of excising teeth, 244
Fire in theatres, security from, 468

Fischer, (F. W.,) on the application of a ligature to fruit trees &c., 499
Fluate of lime, notice of the phosphorence of, 232
Fox, (Mr. D.) account of his elastic mould for casting, 225
Fresnel, (Mr.) elementary view of the undulatory theory of light, 127, 441
Fruits, on the cultivation and improvement of, 265
Fyfe, (Dr. A.) his manual of chemistry, remarks on, 70

Gaertner, (M.) on the fecundation of vegetables, 497
Gaetano Savi, (Prof.) on an Italian cavern containing bones, 241
Gas from resin, its application to a condensed gas lamp, 217
Gas, transference of heat by change of capacity in, 474
Gases, compression of, 467
Gaultier de Claubry, (M.) on the mode of action of disinfecting chlorides, 237
Gelatin of bones, observations on the use of, as an article of food, 56, 386
Gimbernat, (M. Charles de) on the nutritious portion of bones, 56, 386
Giraffe, the, on its preference of milk as a food, 493
Glass, method of cutting, 227
Goring (C. R., M.D.) on artificial cobwebs for micrometers, 81—on achromatic microscopes, 410
Granville, (Dr.) his remarks on chemical disinfectants, 371
Gray, (Mr.) on the production and preparation of pearls, 493
Grey, (Mr.) corrected translation of his Enchorial manuscripts, 402
Gymnastic exercises, their effects on the form, &c., 319

Harwood, (Dr. J.) his remarks on the structure and habits of the seal, 71.
Heat evolved from air by compression, 228
Henbane and Belladonna, preparation of, 493
Henderson, (Mr.) on a corrected method of computing an observed occultation, 434—on the solar tables, 438
Hieroglyphical fragments, notices of, 402
Hildreth, (Dr.) on the effects of cold, 242
Horticultural Society, proceedings of, 217, 317
Human voice, remarks on, 491
Humphrys, (Mr.) on a fluid for biting-in on plates of soft steel, 226

Inclination, change of, of the dipping-needle, 124
Increase, maturity, and decay of trees, signs of, 494
Indian method of varnishing, 227
Insufflation, or passing of air into the lungs, death by, 491
Iodine cyanide of, 480—oxide of, and iodous acid, remarks on, 477
Iron, separation of from manganese, 235—pure proto-salt of, 236

Italian cavern, containing bones, notice of, 241
Ivory, method of etching on, 226
Ivory, (Mr.) on the heat evolved from air by compression, 228

Jenner, (Dr. Edward) life of, 141
Jenour, (M.) on a peculiar structure of shot cartridges, 224

Kaleidophone, a new philosophical toy, observations on, 344

Labour, remarks on the principle of the division of, as applied to Literature and Science, 37
Lamp, night, without a wick, notice of, 210
——, ornamental, notice of, *ib.*
Language, illustrations of the elementary principles of the structure of, 163
Legumine, new vegetable principle, 485
Leslie's apparatus for ascertaining the specific gravity of powder, 226
Liebeg, (Prof.) on the existence and nature of bromine, 233
Life of Dr. Edward Jenner, 141
Ligature, application of, to fruit-trees and vines, 499
Lightning, instance of a returning stroke by, 230—rods and compasses, remarks on, 471
Lindley, (John, Esq.) remarks by, upon the orchideous plants of Chile, 43
Liskovius, (Dr.) his remarks on the human voice, 491
Literature and Science, remarks on the principle of the division of labour as applied to, 37
Locks, principle of security in the various kinds of, 212
Logwood, black dye and ink prepared from, 488

Mac Culloch's (Dr. J.) remarks on malaria, 294
Madder, on the colouring matter of, remarks on, 239
Mangnetic needle, influence of the Aurora Borealis on, 229
Magnetism produced by metals in motion, general account of, 209—of watch-works corrected, 223
Majendie, (M.) on two new kinds of urinary gravel, 246
Malaria, remarks on, 294
Manganese, separation of iron from, in analysis, 235—and soda, sulphates of, 483
" Manual of Chemistry," remarks on, 70
Mayo, (Herbert, Esq.) remarks on his " Outlines of Humany Physiology, 91
" Meleagris Ocellata," (Cuvier) notice of a specimen of, 214
Melons, sugar of, 239
Mercury, depression of in the tube of a barometer, &c., table of, 122

Metallic Disc in motion, its action on a portion of a voltaic conductor, 228
Metals, electro-conducting power of, 470
Meteorological table, 248—503
Metropolis, remarks on the supply of water to the western parts of, 249—remarks on the architecture of, 352
Micrometers, artificial cobwebs for, 81
Micrometrical measurement of Saturn and of Jupiter and its satellites, 220
Microscope, diamond, notice of, 221
Milk, observations on the use of as food, 493
Mould (elastic) for castings, account of, 225

Nakous, subterraneous noises heard at, 240
Nasse (M.) on the preparation of oxide of chrome as a pigment, 235
Natural History, remarks on the study of, 83
Nature, numerical divisions in, 362
Naval architecture and nautical economy, essays and gleanings on, 18
Navier, (M.) experiments on the resistance of various bodies to rupture caused by longitudinal tension, 223
New York, remarks on the subject of the weights and measures of, 101
Nobili, (M.) on electro-chemical appearances, 229
Numerical divisions in nature, 362

Oils, essential substances contained in, 486
Ornithological system of Cuvier and Dumeril, revised arrangement of, with reference only to the genera and species of the British islands, 286
Ottley's Dictionary of Chemistry, remarks on, 54
Oval, beautiful forms derived from, 457
Oxalates, action of potassium on, 236
Oxide of chrome, preparation of as a pigment, 235—of iodine and iodous acid, remarks on, 477—of iron, (native) presence of ammonia in, 481
Ozmazone, notice of, 490

Parkes, (Samuel, Esq.) sketch of the life of, 1
Payen, (M.) his process for the preparation of medicinal chloride of soda, 236—on sugar of melons, 239
Pearls, production and preparation of, 493
Peretti, (M.) on ozmazone, 490
Peru, and Mexico, conquest of by the Mongols in the 13th century, history of, 247
Phillips, (Mr. R.) on the analysis of the triple prussiate of potash, 236
Phosphorescence of fluate of lime, notice of, 232
Physiology, human, outlines of by Mr. Herbert Mayo, 91

Plants, orchideous of Chile, remarks upon, 43—dried, from Massachusetts, notice of, 210—structure of, 496—and vegetables, method of reviving, 500
Ponderable matter, transference of in electric discharges, 472
Potash, analysis of the triple prussiate of, 236—peroxideof, from nitre, 483
Potassium, action of, on oxalates, 236
Powders, apparatus for taking the specific gravities of, 226

Quadrupeds, note on the motions and actions of, 243.
Quesnevile, (M.) on the separation of iron from manganese, 235
Quinia, sulphate of, quantity manufactured, and adulteration of, 484

Rattlesnakes, poison of, 246
Record, (M. le Dr.) his mode of preventing mischief resulting from the bites of venomous reptiles, 246
Reinagle, (Mr.) on the beautiful forms derived from the oval, 457
Renwick, (James, Esq.) on the weights and measures of New York, 101
Reptiles, venomous, method of preventing mischief from the bites of, 246
Resistance of various bodies to rupture caused by longitudinal tension, 223
Returning stroke by lightning, instance of, 230
Roads, Macadamized, report on, 470
Robiquet and Colin, (MM.) on the colouring matter of madder, 239
Rousseau, (M. E.) on the poison of rattlesnakes, 246
Royal Institution of Great Britain, proceedings of, 209, 456—medals, on the recent adjudgment of, by the President and Council of the Royal Society, 11—Society, proceedings of, 207, 455

Sabine (Capt.) extract of his letter respecting the subject of the weights and measures of New York, 382
Salt, on the introduction of as an agricultural agent, 337
Salts, efflorescent of volcanic rocks, 482
Sea, fresh water found at, 502
Sea-gull, measurement and weight of, 244
Seal, on the structure and habits of the, 71
Shot cartridges, peculiar construction of, 224
Silver, crystallized chromate of, 484
Sketch of the Life of the late Samuel Parkes, Esq., 1
Skins, method of preserving, 227
Soft steel, menstruum for biting-in on plates of, 226
Solar tables, remarks on the, 438
Struve, (M.) on micrometrical measurements, 220

Structure of language, illustrations of the elementary principles of the, 163
Subterraneous noises heard at Nakous, 240
Swainson, (W., Esq.) remarks on the importance of preserving facts connected with the natural history of animals, 83

Teeth, excision of, remarks on, 244
Tenacity of metals, observations on, 462
Thames Tunnel, report on the progress and present state of, 466
"The Gold-headed Cane," review of, 278
Tin, method of bronzing, 483
Translation, corrected, of Mr. Grey's Enchorial manuscripts, 402
Turpentine, oil of, its change by exposure to air, 487
Turrell, (Mr.) on the uses of the diamond in the arts, 464

Undulatory theory of light, elementary view of the, 127
Urinary gravel, on two new kinds of, 246

Vapour, new phenomena produced by, 472
Varnishes, improvements in the manufacture of, 488
Varnishing, Indian method of, 227
Vegetable proximate principles, composition of, 485
Vegetables, experiments on the fecundation of, 497
Voltaic conductor, action of a moving metallic disc, on a portion of, 228

Warm-baths, hints respecting the construction of, 62
Watch works, magnetism of, corrected, 223
Water, earthy depositions from, influenced by the electricity of metals in contact, 230
Water, remarks on the supply of, to the western part of the metropolis, by the Grand Junction Water-works Company, 249
——— fresh, found at sea, 502
Weather, natural prognostics of, 501
Weights and measures of New York, report on, 101—remarks on the report, 382
Wheatstone, (Mr. C.) his remarks on the Kaleidophone, 344
White copper, composition of, 483
—— light of the sun, effect of, in making steel magnetic, 125
Witting, (Dr.) his method of destroying the empyreumatic odour of alcohol, 239
Wood, quantity of charcoal from, 479
Wounds, mode of treating when poisoned, 245

Zero, change of in thermometers, 475

END OF VOLUME I.

1827-1830. VII Vols.
Duplicate.
March 1880.

16/4 P.

The Binder *of the Volume* is requested to cancel the *duplicate* Table of Contents of No. II., and to place the Notices to Readers and Correspondents in their proper order, next to the Contents of the Volume.

London: Printed by W. Clowes, Stamford Street.